THURSDAY, MARCH 27, 1873

UNIVERSITY OARS

I.

WE have, not without motive, adopted the title of Dr. Morgan's book,*—so opportunely timed in its issue by its University publishers—as the heading for some considerations connected with the coming river "Derby;" for we propose to pass in review the leading features of the Hygienic value of these contests, which are claiming and receiving from year to year a growing importance, into which the book itself is an exhaustive inquiry.

Many a strong hand will tremble as it lifts this book for the first time, and many an eye will glisten with pleasure or grow dim with regret as it scans its lists and tables and reads the revelations made therein. For what do they tell—and tell too with a rare fulness and circumstantiality? All particulars as to the health, past and present, of the Oarsmen of both Universities who have rowed in the annual matches during the last forty years; that is, from the time of their organisation up to the last race rowed before the author began collecting the materials for his book. Year by year the crews are formed and the races rowed. Year by year the races pass and are forgotten, and the crews disappear and are *not* forgotten, although they may pass away from our sight. What has become of the old Oarsmen, the friends and favourites of other days? Are they "doing duty" in peaceful country parishes, or in crowded cities at home? or have they venturously gone forth to new lands to seek for more genial employments than the old one yields? What are they doing now? how fares it with them? and above all, have they suffered in heart or brain, in nerve or lung, from their old practice at the oar? The ample lists in Dr. Morgan's book, his own ably written pages, and the liberal extracts from his correspondents' answers to his queries—his correspondents being the oarsmen themselves and his queries being with sole reference to their health and bodily condition—tell us all: tell us where they are, what they are doing, what they did when with us and how they did it; and, in their own language, tell with characteristic frankness, and in words which we can still recognise as their old modes of expression, what they think and believe for or against their old favourite pastime. All write cheerily, and all to a man almost speak with prideful remembrance of their work at the oar, and the good they have derived from it. From Bengal writes McQueen:—"I am now a stout man, weighing fifteen stone, but able to be in the saddle all day without fatigue, or if necessary walk my ten or fifteen miles without any distress." We wonder if he still possess the same hand-power that he had in his youth? He had simply the strongest hand and wrist we have ever known, and never did we place our own palm in his without setting our teeth close, and subjecting the member when set free to a gentle manipulation, to restore circulation and revive feeling in its flattened digits. His was the true Herculean build. Nind writes from Queensland on "Since taking my degree in 1855 my constitution has been put to the test in many climates, for I have lived in Canada, on the west coast of America, and in Australia, and I can safely aver that I never have in trying circumstances found a failure of physical power; and that when hard pressed by fatigue and want of food, the recollection of the endurance developed by rowing and other athletics gave me fresh spirit and encouragement." And yet Nind was not naturally a powerful man. His frame was the very antithesis to that of McQueen. Those who remember him as he first came to the University will recall his exquisitely moulded features, almost feminine in their softness and sweetness of expression. Schneider writes from New Zealand:—"I may state that so far as I am concerned, I am able to discover no particular symptoms either good, bad, or indifferent specially attributable to rowing. . . . . I now come to what I believe to be the chief if not the only real danger attendant upon Boat-Racing, and that is the violent strain upon the action of the heart caused by rowing a rapid stroke and exerting every energy to maintain the same to the end of the race."

Who among us could argue the matter more wisely? These are bright and pleasant pictures, but like all other pictures, they have their dark side. In the lists of Oarsmen certain names are printed in italics—not many, thank God!—a small percentage only. These are they who have rowed out their life-race; who have for ever passed out from their period of training and of trial. They rise before our mind's eye as we first knew them. Brewster's magnificent form towering half a head above his stalwart shipmates. Men are all wise after the events; and we hear now of those who always doubted his real strength and stamina, and point to his untimely end as evidence of their own penetration. "Invalided from his regiment, caught cold by returning wet from a Brighton Volunteer Review: died from its effects." Polehampton, the chivalrous, the gentle, the brave! "Decorated while at college with the Royal Humane Society's Medal for saving a companion from drowning at his own imminent peril. Shot through the body at Lucknow—and died of cholera when attending to his comrades stricken by the same malady." The very career he would have marked out for himself, had it been left to his hand to trace it! Hughes, the accomplished, the frank, the manly—the very nature that, speaking in our love and in our pride, we emphatically style the beau-ideal of an English gentleman—"died last year of inflammation of the lungs." Here our personal reminiscences of old oarsmen must cease.

For many a long year strange tales of the risks and dangers of rowing, or rather of boat-racing, have had a floating existence in the Universities, and gaining strength and circumstantiality by time and repetition, have extended to wider circles. While the old tales lived and held their own, other and more startling legends sprang up, and also grew into importance, legends so alarming, and related with such circumstantial detail, that the most sceptical began to think that "there must be something in it." Whole crews, it was stated, had been swept off in a few brief years by their terrible struggles and efforts at the oar. This feeling of uneasiness, if not of absolute alarm, attained a sort of climax a few years ago by the letters of an eminent surgeon, published in the *Times*.

* "University Oars." By John Ed. Morgan, M.A. Oxon, F.R.C.P. (Macmillan, 1873.)

For reasons which seemed to his professional judgment sufficient, he took the side of the alarmists, and pronounced an opinion, strongly expressed, against boat-racing as now practised. These letters were answered with more or less ability by votaries of the oar, men then actively engaged in rowing, or who had recently been so. The controversy lasted for some time, and at last rather died out, or was allowed to drop, than brought to any satisfactory conclusion by the arguments or proofs advanced on either side. By the opponents of boat-racing the case was opened rather unguardedly by statements requiring a stronger array of facts than could be brought to support them when the call for proofs was made; by its defenders was met by the somewhat blunt rejoinder, "You don't know anything about it; you never lifted an oar in your life." The former forgetting that there is nothing so difficult to overcome as enthusiasm, *esprit de corps*, and, perhaps, prejudice; the latter forgetting that the effects of certain modes of exertion on certain organs and tissues of the human body may be sagely divined by a skilful and experienced physician or surgeon, without his ever having in his own person practically undergone such exertion.

As we have said, the argument dropped rather than was brought to any satisfactory conclusion, and if each side did not claim the victory, each stoutly denied that the other had won. Unto this day do we hear alarms sounded with reference to these races, again does paterfamilias feel nervous qualms at the intelligence that his son has betaken himself to the river. Again do non-rowing men console themselves for the want of river distinctions by the thought of their exemption from its risks and liabilities, and again do rowing men enjoy the *éclat* of having greatly dared for the reputation of their Colleges and University, with the secret conviction and comfort that the dangers they have run have been very slight indeed.

It was to close this open question for ever, and settle once for all this standing dispute which has many scientific aspects of great interest, that Dr. Morgan undertook the present work, recognising evidently to the full the standpoint selected by the disputants in the controversy, the one, their practical knowledge as experienced oarsmen, the other, his theoretical knowledge as a scientific surgeon; for, as the author informs us, his qualifications for the task are twofold:—

"As a physician to a large hospital, I have necessarily enjoyed large opportunities of gaining an insight into the laws which regulate our health, while my rowing experience began at Shrewsbury (where I spent many a pleasant hour on the Severn), and was matured at University College, Oxford, where I was for three years Captain of the John +, a boat which has often played a prominent part in the struggles on the Isis, and which has served as the training school for no fewer than ten of the crews which during the last thirty years have won the University Fours."

These qualifications certainly seem adequate to the task, and the plan pursued by Dr. Morgan also seems the best possible, albeit entailing enormous labour on, and demanding vast patience from, him. This plan was simply to institute a strict and exhaustive search after all the men who have rowed in these inter-University contests; to track them, as it were, to whatever part of the world they may have gone: this done, to get their own written testimony, if alive, and that of their friends, if dead, as to whether the part they played in these contests entailed any after evil results upon their constitutions and frames, and (if any) their nature and extent.

Considering that more than forty years have elapsed since the commencement of these friendly contests, and that between the years 1829–1869 twenty-six races have been rowed, giving for the crews of both Universities, and allowing for men who have rowed in more than one race, the gross number of 294 men, the task was a formidable one; but, we must add, has been as ably conducted to its conclusion as it was resolutely undertaken. The author has ascertained that out of these 294 men 245 are still living—39 having died: the time of their death and the ailment of which they died are carefully given by the author, and to this point we will return. He next tabulates the following results elicited by his inquiries:—

Benefited by rowing	. . .	115
Uninjured „	. . .	162
Injured „	. . .	17

The *benefits* derived are somewhat vaguely stated, as indeed was to be expected when almost the only benefits that could be reasonably derived from such pursuits would be of a *general* nature; such as increase of strength and stamina, increase of energy to undertake, increase of power to undergo physical exertion; increase of fortitude to encounter and to submit to trials and privations and disappointments. A goodly list of benefits when critically examined. *The uninjured* are those who in their replies to the author's queries state *negatively* the results of rowing upon their constitutions and frames; or, in the author's language, who merely say in general terms "that they never felt any inconvenience from rowing;" while the *injured* are they who state with less or more distinctness that their exertions proved harmful.

We must confess that this last item in the bill, the 17 injured, is at first sight a little startling, and so it must, we think, have appeared to the author, for he very carefully and minutely examines the cases so recorded, and some, we think, successfully dismisses as unreal; while others, we fear it must be candidly avowed, must remain as *bonâ fide* instances of injury. But is this a matter to be wondered at when the number of men who had been so engaged is taken into consideration? Is there any other pastime or pursuit in which grown men can take part, such as draws forth at the same time their bodily power and keenest emulations, which will yield a smaller percentage of evil? Would the hunting-field, would the foot-ball field, or even the cricket-field, if closely scrutinised?

The author tells us that during his inquiries on this subject he has written over two thousand letters. We can well believe it, knowing how unwilling many men are to reply to personal inquiries, and specially so when the inquirer asks after personal ailments. He has not however done himself justice in not giving us in his book a specimen of his letters addressed to his scattered correspondents; for in all cases of dispute, and contested evidence, it is always a matter of objection if the question as put indicates or leads up to the sort of answer desired; and when, as has been already said, scepticism on one side and *esprit de corps* on the other so strongly prevails, doubts may be entertained of the accuracy of some of

the statements made in the correspondents' replies. But we think that it will be admitted that as a whole those replies are eminently satisfactory.

A circumstance quite noteworthy, however, strikes the reader who scrutinises the lists as tabulated recording the instances of *injured*, and we would be glad to hear some explanation or interpretation of what at present seems inexplicable. Thus out of the first six races only three men are recorded as injured, while out of the next four races nine men are so recorded, five being mentioned in one race—that of 1845—and two more in the race of the following year. Again occurs a period of comparative immunity from injury, only one case being instanced in the next seven races. Once more is the order changed, for in the following four races four men are recorded as injured, while in the five remaining races of the series no injury whatever seems to have been sustained. The author does not seem to have instituted any inquiry on this point, yet surely it is one worth investigation, seeing that it is this very matter of liability to injury which is the sole subject of dispute, to settle which is the avowed object of his book. Was this injury-rate affected by the mode of training of the crews, the physical calibre or age of the individual men composing them, by the severity of the contest itself, or by the character of the season when the men trained and rowed?

ARCHIBALD MACLAREN

THOMSON & TAIT'S NATURAL PHILOSOPHY

Elements of Natural Philosophy. By Professors Sir W. Thomson and P. G. Tait. Clarendon Press Series. (Macmillan and Co., 1873.)

NATURAL Philosophy, which is the good old English name for what is now called Physical Science, has been long taught in two very different ways. One method is to begin by giving the student a thorough training in pure mathematics, so that when dynamical relations are afterwards presented to him in the form of mathematical equations, he at once appreciates the language, if not the ideas, of the new subject. The progress of science, according to this method, consists in bringing the different branches of science in succession under the power of the calculus. When this has been done for any particular science, it becomes in the estimation of the mathematician like an Alpine peak which has been scaled, retaining little to reward original explorers, though perhaps still of some use, as furnishing occupation to professional guides.

The other method of diffusing physical science is to render the senses familiar with physical phenomena, and the ear with the language of science, till the student becomes at length able both to perform and to describe experiments of his own. The investigator of this type is in no danger of having no more worlds to conquer, for he can always go back to his former measurements, and carry them forward to another place of decimals.

Each of these types of men of science is of service in the great work of subduing the earth to our use, but neither of them can fully accomplish the still greater work of strengthening their reason and developing new powers of thought. The pure mathematician endeavours to transfer the actual effort of thought from the natural phenomena to the symbols of his equations, and the pure experimentalist is apt to spend so much of his mental energy on matters of detail and calculation, that he has hardly any left for the higher forms of thought. Both of them are allowing themselves to acquire an unfruitful familiarity with the facts of nature, without taking advantage of the opportunity of awakening those powers of thought which each fresh revelation of nature is fitted to call forth.

There is, however, a third method of cultivating physical science, in which each department in turn is regarded, not merely as a collection of facts to be co-ordinated by means of the formulæ laid up in store by the pure mathematicians, but as itself a new mathesis by which new ideas may be developed.

Every science must have its fundamental ideas—modes of thought by which the process of our minds is brought into the most complete harmony with the process of nature—and these ideas have not attained their most perfect form as long as they are clothed with the imagery, not of the phenomena of the science itself, but of the machinery with which mathematicians have been accustomed to work problems about pure quantities.

Poinsôt has pointed out several of his dynamical investigations as instances of the advantage of keeping before the mind the things themselves rather than arbitrary symbols of them; and the mastery which Gauss displayed over every subject which he handled is, as he said himself, due to the fact that he never allowed himself to make a single step, without forming a distinct idea of the result of that step.

The book before us shows that the Professors of Natural Philosophy at Glasgow and Edinburgh have adopted this third method of diffusing physical science. It appears from their preface that it has been since 1863 a text-book in their classes, and that it is designed for use in schools and in the junior classes in Universities. The book is therefore primarily intended for students whose mathematical training has not been carried beyond the most elementary stage.

The matter of the book however bears but small resemblance to that of the treatises usually put into the hands of such students. We are very soon introduced to the combination of harmonic motions, to irrational strains, to Hamilton's characteristic function, &c., and in every case the reasoning is conducted by means of dynamical ideas, and not by making use of the analysis of pure quantity.

The student, if he has the opportunity of continuing his mathematical studies, may do so with greater relish when he is able to see in the mathematical equations the symbols of ideas which have been already presented to his mind in the more vivid colouring of dynamical phenomena. The differential calculus, for example, is at once recognised as the method of reasoning applicable to quantities in a state of continuous change. This is Newton's conception of Fluxions, and all attempts to banish the ideas of time and motion from the mind must fail, since continuity cannot be conceived by us except by following in imagination the course of a point which continues to exist while it moves in space.

The arrangement of the book differs from that which has hitherto been adopted in text-books. It has been

usual to begin with those parts of the subject in which the idea of change, though implicitly involved in the very conception of force, is not explicitly developed so as to bring into view the different configurations successively assumed by the system. For this reason, the first place has generally been assigned to the doctrine of the equilibrium of forces and the equivalence of systems of forces. The science of pure statics, as thus set forth, is conversant with the relations of forces and of systems of forces to each other, and takes no account of the nature of the material systems to which they may be applied, or whether these systems are at rest or in motion. The concrete illustrations usually given relate to systems of forces in equilibrium, acting on bodies at rest, but the equilibrium of the forces is established by reasoning which has nothing to do with the nature of the body, or with its being at rest.

The practical reason for beginning with statics seems to be that the student is not supposed capable of following the changes of configuration which take place in moving systems. He is expected, however, to be able to follow trains of reasoning about forces, the idea of which can never be acquired apart from that of motion, and which can only be thought of apart from motion by a process of abstraction.

Profs. Thomson and Tait, on the contrary, begin with kinematics, the science of mere motion considered apart from the nature of the moving body and the causes which produce its motion. This science differs from geometry only by the explicit introduction of the idea of time as a measurable quantity. (The idea of time as a mere sequence of ideas is as necessary in geometry as in every other department of thought.) Hence kinematics, as involving the smallest number of fundamental ideas, has a metaphysical precedence over statics, which involves the idea of force, which in its turn implies the idea of matter as well as that of motion.

In kinematics, the conception of displacement comes before that of velocity, which is the rate of displacement. And here we cannot but regret that the authors, one of whom at least is an ardent disciple of Hamilton, have not at once pointed out that every displacement is a vector, and taken the opportunity of explaining the addition of vectors as a process, which, applied primarily to displacements, is equally applicable to velocities, or the rates of change of displacement, and to accelerations, or the rates of change of velocities. For it is only in this way that the method of Newton, to which we are glad to see that our authors have reverted, can be fully understood, and the "parallelogram of forces" deduced from the "parallelogram of velocities." Another conception of Hamilton's, however, that of the hodograph, is early introduced and employed with great effect. The fundamental idea of the hodograph is the same as that of vectors in general. The velocity of a body, being a vector, is defined by its magnitude and direction, so that velocities may be represented by straight lines, and these straight lines may be moved parallel to themselves into whatever position is most suitable for exhibiting their geometrical relations, as for instance in the hodograph they are all drawn from one point. The same idea is made use of in the theorems of the "triangle" and the "polygon" of forces, and in the more general method of "diagrams of stress," in which the lines which represent the stresses are drawn, not in the positions in which they actually exist, but in those positions which most fully exhibit their geometrical relations. We are sorry that a certain amount of slight is thrown on these methods in § 411, where a different proposition is called the *true* triangle of forces.

It is when a writer proceeds to set forth the first principle of dynamics that his true character as a sound thinker or otherwise becomes conspicuous. And here we are glad to see that the authors follow Newton, whose Leges Motûs, more perhaps than any other part of his great work, exhibit the unimproveable completeness of that mind without a flaw.

We would particularly recommend to writers on philosophy, first to deduce from the best philosophical data at their command a definition of equal intervals of time, and then to turn to § 212, where such a definition is given as a logical conversion of Newton's First Law.

But it is in the exposition of the Third Law, which affirms that the actions between bodies are mutual, that our authors have brought to light a doctrine, which, though clearly stated by Newton, remained unknown to generations of students and commentators, and even when acknowledged by the whole scientific world was not known to be contained in a paragraph of the *Principia* till it was pointed out by our authors in an article on "Energy" in *Good Words*, October 1862.

Our limits forbid us from following the authors as they carry the student through the theories of varying action, kinetic force, electric images, and elastic solids. We can only express our sympathy with the efforts of men, thoroughly conversant with all that mathematicians have achieved, to divest scientific truths of that symbolic language in which the mathematicians have left them, and to clothe them in words, developed by legitimate methods from our mother tongue, but rendered precise by clear definitions, and familiar by well-rounded statements.

Mathematicians may flatter themselves that they possess new ideas which mere human language is as yet unable to express. Let them make the effort to express these ideas in appropriate words without the aid of symbols, and if they succeed they will not only lay us laymen under a lasting obligation, but, we venture to say, they will find themselves very much enlightened during the process, and will even be doubtful whether the ideas as expressed in symbols had ever quite found their way out of the equations into their minds.

TYNDALL'S FORMS OF WATER

The forms of Water in Clouds and Rivers, Ice, and Glaciers. By John Tyndall, LL.D., F.R.S. (London: H. S. King & Co.)

WHATEVER comes from Dr. Tyndall's pen is sure to be vivid and clear. The present little volume forms no exception to this rule. It seems to have been composed partly in the form of popular lectures and partly as a sort of journal of a visit last year to the author's favourite holiday haunts among the Swiss glaciers. Very readable, it nevertheless betrays this composite origin, and wears more the aspect of a piece of book-making than

probably its author himself could have wished. A wrong impression of the subject is created by the title, which though singularly happy in itself does not fairly describe the contents of the book. Such a title suggests an accurate and luminous discussion of the phenomena of evaporation and condensation, the growth and movements and disappearances of mists and clouds, the formation and distribution of rain and the laws regulating the rainfall over the globe, the meaning of frost, the birth and history of hail and snow, the circulation of water over the land with the ways and workings of brook, stream, and river, from mountain-peak to sea-shore, the architecture and the functions of snow-fields, glaciers, and icebergs—in short a kind of scientific poem, dedicated to the glory of that grand old element—water. Dr. Tyndall could write such a poem better than most men, and indeed it was with the expectation that he had attempted it that we opened this last volume of his. Out of the 192 pages 28 are devoted to clouds, rains, rivers, waves of light and heat, oceanic distillation and mountain condensers. The rest treat wholly of ice. So that if we may judge by the relative space devoted to the different forms of water, ice must be six times more important than all the rest put together. A less ambitious title, such as its author could readily suggest, descriptive of the fact that the book is a record of work, intellectual and corporeal, among the Swiss glaciers, would prevent the feeling of disappointment with which many a reader has no doubt come to the last page.

Dr. Tyndall did not intend, we suppose, that his book should be regarded in any other light than as a popular exposition of his subject, and would probably disclaim any place for it as a contribution of new facts and reasonings to our knowledge of glaciers. His narratives of last year's climbings and observations read very much like those of older ones with which he has already made us familiar. They are pleasantly written, and will convey to a reader, who has never seen a glacier, a picturesque notion of what he has missed. But surely it was not necessary to rake up again the Forbes-Rendu controversy, nor to renew the claims of Agassiz and Guyot. We could have wished, too, that while alluding to Mr. Mathews and other recent observers on ice-structure the writer had taken some notice of the attack upon his own theory by Canon Mosely and Mr. Croll.

OUR BOOK SHELF

Die Anwendung Des Spectralapparate von Dr. K. Vierordt. (Tübingen : H. Laupp, 1873.)

DR. VIERORDT has been endeavouring to found a quantitative spectrum analysis for bodies giving an absorption spectrum. His method consists in the use of a slit divided horizontally into two parts ; one of these is adjusted to a certain width ; the solution whose absorption is to be examined is placed opposite this, and in front of the other half is placed another solution of the same body but of a different strength, and the slit is then narrowed or widened as the solution is stronger or weaker until the absorption is the same in both halves of the spectrum. The width of the latter slit is then read off. By using a number of solutions of strengths varying decimally from the weakest possible to the strongest through which light will pass, curves are obtained and a solution of unknown strength can then be interpolated in the curve and its value ascertained. The solutions to be examined are, of of course, kept at a constant thickness. As the relation between the concentration of the solution and its coefficient of the absorption of light only remains constant within certain limits, solutions of the necessary dilution have to be employed and unknown solutions must be diluted to this point : the value is then found by calculation.

Tables for calculations of various kinds required are given, and the memoir is illustrated with lithographs of the working details of the divided slit. A number of specimen curves are also given. The memoir is well worthy the attention of all who have to estimate the strength of colouring matter.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Existence of Man in the Miocene

I HAVE received a letter from Mr. Edmund Calvert, in which he informs me that his brother, Mr. Frank Calvert, has recently discovered, near the Dardanelles, what he regards as conclusive evidence of the existence of man during the Miocene period. Mr. Calvert had previously sent me some drawings of bones and shells from the strata in question, which Mr. Busk and Mr. Gwyn Jeffreys were good enough to examine for me. He has now met with a fragment of a bone, probably belonging either to the Dinotherium or a Mastodon, on the convex side of which is engraved a representation of a horned quadruped, "with arched neck, lozenge-shaped chest, long body, straight fore legs, and broad feet." There are also, he says, traces of seven or eight other figures, which, however, are nearly obliterated. He informs me that in the same stratum he has also found a flint flake, and several bones broken as if for the extraction of marrow.

This discovery would not only prove the existence of man in Miocene times, but of man who had already made some progress, at least, in art. Mr. Calvert assures me that he feels no doubt whatever as to the geological age of the stratum from which these specimens were obtained.

Of course I am not in a position myself to express any opinion on the subject ; but I am sure that the statements of so competent an observer as Mr. Calvert will interest your readers.

High Elms, March 23 JOHN LUBBOCK

Adaptation to External Conditions

THE curious modification of adaptation to external conditions in the case of the *Salamandra atra*, which I have more than once brought under the notice of naturalists, but which I myself first noticed under the direction of Prof. von Siebold, has been cited by Mr. Darwin ("Origin of Species," 4th Ed. p. 534) in confirmation of his views. I revert to it now for the sake of its illustration of a new and striking observation, which has excited the incredulity of several eminent naturalists in France—an incredulity, we may suppose, founded on their ignorance of the previous observation. The fact to which I called attention was this : The ordinary salamander, or Newt, is born in the water as a tadpole, and in the water it completes its metamorphosis. But the *Salamandra atra*, living high up in the mountains, with no pools in which to pass its tadpole existence, is born on the land, a completely formed animal ; that is to say it passes through the tadpole stage while still within its mother's body. I have taken it from the gravid female in this tadpole state, and placed it in water, wherein it swam as if that were its natal element.

In the *Revue Scientifique*, No. 37, there has just appeared a brief account of some observations made by M. Baray at Guadeloupe, from which it appears that the frogs, having in that volcanic island no marshes nor pools suitable for the early tadpoles, have adapted themselves to these conditions by passing through

all the tadpole stages of metamorphosis while still in the egg. All these stages have been observed by M. Baray; and whoever is familiar with the evolution of the ordinary tadpole before it quits the egg, will see that M. Baray has observed only a modified form of the well-known process. The Guadeloupe frog is born as a frog, not as a tadpole; and this, paradoxical as it may seem to some naturalists who cannot dismiss traditional conceptions, is even less remarkable than the case of the *Salamandra atra*, because it is only an extension of the period of incubation, whereas with the salamander it is the substitution of viviparity for oviparity. How the presence of water leads to an acceleration of the birth, or the absence of water leads to its retardation, is an interesting point for investigation; whether retarded or accelerated, the finally-acquired structure is the same.

The Priory, March 22 GEORGE HENRY LEWES

Anticipations of Natural Philosophy

MAUPERTUIS

HAVING lately had occasion to examine the works of Maupertuis I, like Prof. Jevons, was struck by meeting with anticipatory glimpses of the modern theory of Natural Selection. The passage, given almost word for word by Lord Bolingbroke in the quotation made by Prof. Jevons, occurs somewhat incidentally in two parts of Maupertuis' writings; in the memoir alluded to ("Les Loix du Mouvement et du Repos, déduites d'un principe métaphysique"); and in the "Essay de Cosmologie," into which the memoir was expanded five years later (1751). In both these works Maupertuis is chiefly concerned with establishing his well-known metaphysico-mechanical principle of "The Least Action" ("La moindre Quantité d'Action"); and with deducing therefrom proof of the existence of God. But the doctrine of "The Survival of the Fittest" is more clearly discernible, and more than incidentally referred to, in his small physiological treatise, "Venus physique" (Œuvres, tome ii. ed. 1756). The whole of this work is not wanting in interest, but as bearing specially on the subject in question, I would mention the third, fifth, and last chapters of the second part. Chapter III. is entitled "Production de nouvelles especes." In it the most pronounced passage is perhaps the following: "Mais la sage Nature, par le dégoût qu'elle a inspiré pour ces défauts, n'a pas voulu qu'ils se perpetuassent; chaque père, chaque mère fait de son mieux pour les éteindre; les beautés sont plus sûrement héréditaires; la taille, et la jambe, que nous admirons, sont l'ouvrage de plusieurs générations, où l'on s'est appliqué à les former." Chapter V., called an "Essay d'explication des phénomènes précédents," is an attempt to explain the physiological processes at work in the preservation of the best types, and in the production of new forms. On the efficacy of these processes the author says: "L'expérience pourroit, peut-être, éclaircir ce point; si l'on essayoit pendant longtemps de mutiler quelques animaux de génération en génération, peut-être verroit-on les parties rétranchées, diminuer peu à peu; peut-être verroit-on les à la fin s'anéantir." The last chapter contains a summary of the whole work, and a number of "Doutes et Questions," propounded by the author. In one of these he asks, "Cet instinct des animaux, qui leur fait rechercher ce qui leur convient, et fuir ce qui leur nuit, n'appartient-il point aux plus petites parties dont l'animal est formé?" In another question Maupertuis puts forward a bold hypothesis as to the influence which the decomposed material of the dead animal organism might exercise upon plants, and through them upon the structure and character of the living organism.

In his *Système de la Nature* also (Œuvres, tom. ii. ed. 1756), Maupertuis combats the special creation theory of the origin of species, and advocates a doctrine, which may be called Natural Selection, the selective principle being placed in the ultimate elements of both organic and inorganic substances, of which elements "la perception est une propriété essentielle," and which "doués d'intelligence s'arrangent et s'unissent pour remplir les vues du Créateur."

Such are a few of the glimpses to be met with in the French philosopher, of the modern doctrine of Darwin and Spencer. Similar ones may not improbably be found elsewhere, but such "resultless tendencies," as the course of events has proved them to be, can in no degree detract from the merit and originality of those who have made of Natural Selection a well-substantiated and homogeneous theory. W. H. BREWER

Grace's Road, Camberwell, March 10

EMPEDOCLES

ON reading Prof. W. Stanley Jevons' interesting letter in this week's NATURE, I referred to my note-book, and found the following quotation, under the title of "Natural Selection," which shows that the opinion of Maupertuis is at least as old as Empedocles.—"Cette dernière opinion sert à expliquer les idées d'Empédocle sur la production des animaux par des causes accidentelles. L'attraction et la répulsion des élémens donnèrent naissance dans les commencemens et par le seul effet du hasard, à des têtes sans cou, à des jambes sans corps, à des animaux moitié bœufs et moitié hommes, en un mot, à une foule de monstrés semblances. Parmi tous ces êtres, les uns étaient construits de manière qu'ils semblaient êtres doués de l'intelligence: ceux-là conservèrent la vie, et propagèrent leur espèce, mais ceux auxquels l'organe de la vie manquait, retombèrent dans le chaos, d'ou ils étaient sortis." ("Histoire de la Medécine," par Kurt Sprengel, vol. i. p. 249.) Sprengel gives the following references:—Aristotle, Physic. Lib. ii., c. 4, p. 465., c. 8, p. 470. Owing to my distance from a public library I have not hitherto had an opportunity of referring to Aristotle; but as Prof. Jevons is more favourably circumstanced, I hope he will consult the original, and if he finds anything which throws further light upon this interesting question, that he will report it to your readers.

Although, as Prof. Jevons remarks, the introduction of the notion of *chance* is erroneous, the speculation shows how thoroughly the Greek Atomists had banished from their explanations of phenomena all reference to first and final causes, anticipating in this respect the modern conception of science. I cannot deny myself the pleasure of quoting the weighty judgment of Bacon upon this point:—"And therefore the natural philosophies of Democritus and others," says Bacon, "who allow no God or mind in the frame of things, but attribute the structure of the universe to infinite essays and trials of nature, or what they call fate or fortune, and assigned the causes of particular things to the necessity of matter without any intermixture of final causes, seem, so far as we can judge from the remains of their philosophy, much more solid, and to have gone deeper into nature, with regard to physical causes, than the philosophy of Aristotle or Plato; and this only because they never meddled with final causes, which the others were perpetually inculcating." (Advancement of Learning, Book iii. chap. iv.)

Waterfoot, March 8 JAMES ROSS

ARISTOTLE

IT is interesting, as Mr. Jevons says, to observe such traces as are to be found in history of theories more or less anticipating the principle of natural selection. But if the instance he cites from Maupertuis fairly represents the last century in this matter, it is chiefly of interest as showing what a little way it is possible to travel on certain roads in twenty-two centuries: for Aristotle discusses the same theory in his "Physics" (ii. 8), and appears to attribute it to Empedocles. "It may be a question," he says, "whether physiological effects which seem to be due to final causes are not really accidental. An organism survived, we may suppose, if it happened to be as a whole constituted in a suitable manner; that is, in a manner in which it would have been constituted by design; organisms otherwise constituted perished and perish still, like the *βουγενῆ ἀνδρόπρωρα* of Empedocles." Now, except that his monsters are certainly not quite so monstrous, I do not see that the "Flattener of the Earth" gets beyond that. At any rate he lags behind Lucretius, who adopts the same theory of "discriminative destruction" (v. 837–877), but applies it, as Mr. Munro points out (on line 855), not merely to monsters but to "regularly organised creatures," either not so gifted as to protect themselves or not so valuable as to be protected by man.

This is, as far as it goes, a theory of natural selection. It is a theory of the survival of the fit, absolutely; but not being a theory of the preponderant survival of the fitter, and not taking adequate account of inheritance, it is not a theory of evolution. Indeed, though Lucretius recognised a constant change in the conditioning circumstances, and therefore in the organisms conditioned (828–836), it was to account for the stability of species that he called in natural selection and not to give a clue to the laws of their variation. That is the direction in which there must have been most room for progress; and traces of such progress may be to be found. Has Mr. Jevons tried Gassendi? C. J. MONRO

Hadley, Middlesex

Fossil Cryptogams *

I DO not propose at present to controvert in detail all the positions taken up by my friend Prof. McNab in his brief communication to your pages on "Fossil Cryptogams" (vol. vii. p. 267), because the time has not yet arrived for doing so. Much more detailed information respecting the subject which yet awaits publication must be had before it can be discussed in a satisfactory manner. I merely wish to avoid leaving the impression, by my silence, that I either admit his supposed facts or accept his inferences. When his paper, to which he refers, was read in Edinburgh, specimens of sections of Calamites of various ages were sent down by me for the purpose of being exhibited to the Botanical Society. This was done by Prof. Dickson, who at the same time expressed his preference for my views over those of Dr. McNab, as is stated in the officially published notice of the meeting in question. Since then I have received a kind letter from Dr. Balfour, who has carefully examined the specimens referred to, and who also expresses a similar conviction. I think that I have unmistakeable proof of the circumferential growth of Calamites, which Dr. Macnab denies, in specimens of large size, and in which the exogenous zone is of great thickness.

Prof. McNab speaks of "the moist nature of the soil in which the Calamites must have grown," as probably causing a different mode of growth in them, to that "circumferential" one which he admits has probably taken place in Lepidodendra, Sigillariæ, and Dictyoxylons; but I beg to suggest that we have no reasons for thinking otherwise than that these plants grew side by side, and under precisely the same physical conditions, hence the "moist soil" of my friend is an assumption. This close association of Calamites with Sigillariæ was demonstrated and commented upon by Mr. Binney many years ago. Dr. McNab further separates Lepidodendron from Sigillaria and Stigmaria, placing them in different groups. When he receives my third memoir in the Philosophical Transactions (which is printed but not yet circulated), he will see how utterly this plan of procedure is opposed to the facts. I contend that Sigillariæ are virtually Lepidodendra, and that Stigmaria is equally the root of both. As to the location of my old, but now abandoned genus, Dictyoxylon, the more I study it the less I feel competent to fix its true place amongst the Cryptogams. But notwithstanding Dr. McNab's idea as to its coniferous affinities, I venture to affirm, from a prolonged study of a cabinet full of specimens, that its woody axis is not one bit more exogenous than those of Calamites and of *matured* Lepidodendra. The fact is that whatever the vessels of these various exogenous woody zones signify, they must stand or fall together. They are either all ligneous or they are all cortical. I think that my forthcoming illustrations of the *bark-structures* amongst the Burntisland Lepidodendra, as well as of our Lancashire specimens, will show that all the elements which Dr. McNab finds in Lycopodium Chamæcyparissus are present, in their proper places, the schlerenchyma of the hypoderm being especially well represented, yet it is precisely this hypoderm with which Dr. McNab believes my exogenous layer to correspond. There is one if not two distinct layers of cortical parenchyma between this schlerenchymatous layer and my ligneous zone, which latter is so magnificently represented in these plants.

The intimate structure of these latter layers, whether we regard the forms and arrangements of the entire woody wedges or that of their component tissues, is so identical in the two cases of Calamites and Lepidodendra, that an active imagination alone can make the one axial and ligneous, and the other cortical. Dr. McNab draws a distinction between vessels representing ("feebly") the fibro-vascular bundles of the living Equisetums, in the Calamites, and the more external portions of each woody wedge, which he regards as representing the hypodermal schlerenchyma of Mettenius. I unhesitatingly avow that there is no ground whatever for this arbitrary separation. He is putting asunder things which have been joined together from the beginning of time. The tissues in question are as identical in their structure as they are uninterruptedly continuous in their arrangement.

Whilst I am thus opposed to Dr. McNab both on questions of fact and of inference, I feel obliged to him for calling my attention to this possible explanation of the facts, even though after a careful study of his views I feel constrained to reject them so far as the interpretation of Calamites are concerned. On the questions relating to Meristem growths, we are much nearer to mutual agreement, and I accept thankfully his admiss on of the coniferous affinities of Dictyoxylon, not because I am prepared to recognise any specially close coniferous relationships, but because Dr. McNab's idea necessarily involves an admission of the existence of exogenous features in these plants; yet I contend that the Dictyoxylons are neither more coniferous nor more exogenous than most of the other Cryptogamic carboniferous stems which exhibit equally strong proofs of a similar exogenous growth. But I again repeat that we shall not be in a position to grapple philosophically with these problems until all the results of my prolonged researches are published. This is being accomplished as rapidly as my limited leisure admits of. When completed, I shall be quite prepared to enter, if necessary, and in a friendly spirit, upon the entire controversy.

W. C. WILLIAMSON

Owens College

* We regret that the insertion of this letter has been so long delayed in consequence of the great pressure upon our space.

Leaf Arrangement

AFTER reading Dr. Airy's paper on Phyllotaxis (NATURE, vol. vii. p. 343), I cannot see that we are at all nearer than before, any satisfactory explanation as to the inherent cause of it. Let the question be put thus:—If we can conceive, as all will admit, the possibility of leaves being scattered anyhow along a branch, why are they not so, but in some strictly mathematical order? Any disturbance in that order is usually so slight and trivial (due apparently in part to the *conical* nature of the axis, and unequal growth or slight twists; and which thereby cause certain leaves to assume slightly wrong positions), that it does not destroy the fact that they absolutely are arranged, and can be represented, *mathematically*.

In my paper on the angular divergences of the Jerusalem artichoke (Linnean Trans. vol. xxvi. p. 647), I pointed out that two questions might represent all that is required to be solved. (1) That if a leaf be selected as No. 1, then No. 2 lies within a certain arc, viz.:—120°—180° from No. 1, for the ordinary series of fractions, and which it does not transgress—why is this? (2) If we allow that arc—why does the second leaf not assume *any* spot, but is rigidly confined to a certain angular distance from the first?

I cannot think with Dr. Airy that "the way in which all the spiral orders may have been derived from one original order [was] by means of different degrees of twist in the axis." For if we take a piece of round elastic as he describes, with balls fixed according to some spiral arrangement—say $\frac{2}{5}$—then the successive balls will lie at an angular distance of 144°; and if No. 1 be fixed and we twist the indiarubber at No. 2, we may cause it to make a complet rotation if we choose.

If, now, his idea of "twist" be admitted as a *vera causa* of phyllotaxis, we may ask, *what causes the twist to be just so much and no more as to make* No. 2 *pass through* 9° (the angular divergence of $\frac{2}{5}$ being 135), so as to pass into the next $\frac{2}{5}$ arrangement? To say that some such point is a "position of maximum stability" seems to me to give a fictitious importance to the idea of twist, for the expression conveys no really explanatory meaning at all.

Again, to admit that it does not accurately hit the right place, and is in consequence more like Nature, is equally delusive, for Nature is quite accurate enough to be represented mathematically, whereas the positions taken up by the balls must be arbitrary, or at least in proportion to the twist given by the hand—a perfectly arbitrary force? Moreover he appears to overlook the fact that if an axis becomes twisted the *fibres* will be twisted also, but they are not so; the elastic band he adopts would, if it were a pliant shoot, contort the vessels and wood fibres, a condition not obtaining in nature.

Nor can I agree with him in deducing all the members of the series from $\frac{1}{2}$. My experience leads me to infer they are derived from *opposite* leaves, such as one finds in the cotyledons. In the Jerusalem artichoke opposite leaves are frequently succeeded by $\frac{1}{3}$; and this is obtained by the pair of leaves, next above the strictly opposite pair, converging to one side, the next pair do so still more, when it will be found that the $\frac{1}{3}$ arrangement will be henceforth established; the internodes having become more and more developed at the same time.

I strongly suspect the original arrangement to have been whorled and quincuncial. This is at least very abundant, if not universal, in coal plants. The whorls may have subsequently become reduced to fours, threes, and twos or decussate. We see this tendency to symmetrical reduction in many existing plants, *e.g.* stamens and carpels of *Cruciferæ*: *Circæa* as com-

pared with *Epilobium*; the stamens of *Geranium* as compared with *Erodium*. Where the reduction has been unsymmetrical, I suspect it has been due to insect adaptation: as in di-dynamous stamens.

As soon as decussate leaves are secured, then we possess the basis for all ordinary leaf-arrangements.

Dr. Airy alludes to non-existing orders, $\frac{1}{4}$, $\frac{3}{7}$, $\frac{4}{9}$, $\frac{4}{11}$, but in the Jerusalem artichoke the secondary series $\frac{1}{3}$, $\frac{1}{4}$, $\frac{2}{7}$, $\frac{3}{11}$, $\frac{5}{18}$, occurs frequently, and arises from the breaking up of "tricussate" whorls in an exactly similar manner to the primary series, $\frac{1}{2}$, $\frac{1}{3}$, $\frac{2}{5}$, &c., arising out of opposite leaves. On the other hand spirals do not easily, if ever, return to whorls. If any one will notice how curiously the above is executed in the Jerusalem artichoke, he will see that there is evidently some power at work in the plant which, as it were, compels the spiral to form, and to form mathematically, will be convinced, I am sure, that a "twist" is very far from being the cause—there being none whatever in the cases mentioned above: and further, when whorls break up, the leaves are at first quite irregular, but they gradually "right themselves," acquire the proper angular divergence, and then form some member of the spiral arrangements to perfection.

GEORGE HENSLOW

Flight of Projectiles

IN reply to the letter of "W. Hope," in NATURE of March 13, I request permission to state that by a *simple* formula, I meant one that would be easily understood. I did not intend the word simple to be taken strictly in its mathematical sense.

It is easy for Mr. Hope to employ symbols to represent the initial velocity, angle of elevation, or any other additional particular he may consider necessary for the solution of my problem.

No one possessing the most elementary knowledge of the theory of projectiles can be ignorant of the disturbing elements to which your correspondent refers, or of others to which he makes no allusion. But these cannot be accurately estimated, and, therefore, must necessarily be neglected in a theoretical investigation. I do not anticipate that they will be found to vitiate the results of theory to the extent Mr. Hope supposes.

In the practical application of the formula for which I have asked, the numerical values of the general symbols, would be the *mean* of carefully conducted experiments. Thus the trifling variations arising from slight differences in the charge, the amount of fouling, or other causes, would be reduced to a minimum. The variations in the force and direction of the wind would often neutralise each other. For these reasons I cannot agree with Mr. Hope in thinking that the calculation would be either "useless or deluding," on the contrary I believe it would be valuable as indicating a mean deflection, about which the experimental deflections would be found to group themselves.

Of one thing I am certain, that it would enable us to bring home to the soldier the great effect of wind in deflecting the bullet, and perhaps it might assist us in dispelling the notion of absurdity which is inseparably associated in his mind with the effort to hit something by aiming at nothing. In accomplishing this one of the greatest obstacles to the development of skill in rifle-shooting would be removed.

If Mr. Hope will kindly supply me with the formula which I have asked for, I can assure him that however lightly he may appreciate the results of his labours, by me, at least, they will be valued, and, I venture to hope, made practically useful. Surely he cannot be in earnest in denouncing all theory which approximates to, but does not exactly accord with practice, as "bastard science, or pedantry." If this *dictum* be sound, I can only say it would be easy to show that a great deal of the science of our day, gunnery science in particular, is spurious.

General Didion, a high authority, did not consider my problem unworthy of investigation. In the *Cours Elementaire De Balistique*, he has given a solution which I regret is rather too complicated for my purpose. I should imagine that he would be the last person to expect his theory to afford more than a rough approximation to the results of practice. Hence I conclude that in publishing his calculation for the benefit of the French army, he could have had no conception that his science was "bastard science, or pedantry," and must have been unconscious what a "mischievous unpractical pedant" he was.

ROBERT REID, Sergeant-Major

School of Musketry, Hythe, March 17

Deep Sea Soundings near the Equator

THE following extract from a letter of the captain of the school-ship *Mercury*, occupied at present in taking deep-sea soundings under the orders of the Board of Commissioners of Public Charities and Correction of New York, has been sent to me by General Bowen, of that Board, who takes much interest in the subject. It will doubtless be gratifying to many of your readers:—

"Our Casella-Miller deep-sea thermometer worked admirably. This beautiful instrument stood the test at a depth of 2,040 fathoms, two miles north of the Equator, in longitude 22° 16′ W., when it indicated a temperature of 35° F.; at 1,000 fathoms 38°; at 400 fathoms 41°; at 300 fathoms 44°; at the surface 81°; in the air 80°.

"On our track from the Canary Islands to Rio we found the temperatures at uniform depths to vary about 2°. Our specimens of the bottom from the volcanic region differ in every respect from those obtained in other parts of the ocean."

JOHN WM. DRAPER

University, New York, March 6

SURVIVAL OF THE FITTEST

THE doctrine of the "survival of the fittest" must be strangely understood in some quarters. The American papers report Prof. Agassiz as having expressed himself in this wise at a recent meeting of the Massachusetts State Board of Agriculture, of which he is a member:—"I do not know how animals originated; a brilliant imagination that of Darwin; a very necessary faculty in the scientist. The sense I know too well to misquote him. Hasty generalising of observation is Darwin all over. Natural selection is out of generation. Natural necessity, what is it? Do we find that only the strong beget families? *Observe plants at the foot of the White mountains, where are large trees, and so up to the summit, where they are mere shrubs.* The weak may and do survive as well as the strong. Ignorance lies at the base of the discussion."

Probably no one naturalist, however eminent, can be expected to know everything, or even all simple things. Can it be possible that Prof. Agassiz supposes (as his argument seems to require) that the dwarf trees in question grow and survive near the top of the mountain, *notwithstanding* they are not the fittest, rather than *because* they are the fittest, for the conditions? And does he conceive the doctrine of natural selection to be founded upon some idea of an abstract fitness, irrespective of the conditions, and not upon the survival of the fittest under and in consequence of the conditions? Surely the argument brought against the doctrine is a good illustration in its favour, only an extremely simple and elementary one.

We never could quite comprehend why Prof. Agassiz should give himself so heartily and persistently to the work of demolishing the doctrine of the derivation of species, in all its forms, considering how large and honourable a part he has himself taken in laying the foundation upon which the modern doctrine has been built. Of these foundations none is stronger than the capital one, generally supposed to be established by him, that the succession of species in time corresponds mainly with that in systematic rank, and is also somehow paralleled in the development of each individual of the higher ranks. So that, in view of his continued but unsuccessful efforts to drive the incoming doctrine out of the land, we could imagine him addressing his own important discoveries in the words used by Balak to Balaam:—"What hast thou done unto me? I took thee to curse mine enemies, and behold, thou hast blessed them altogether."

SUB-WEALDEN EXPLORATION.—SECOND QUARTERLY REPORT

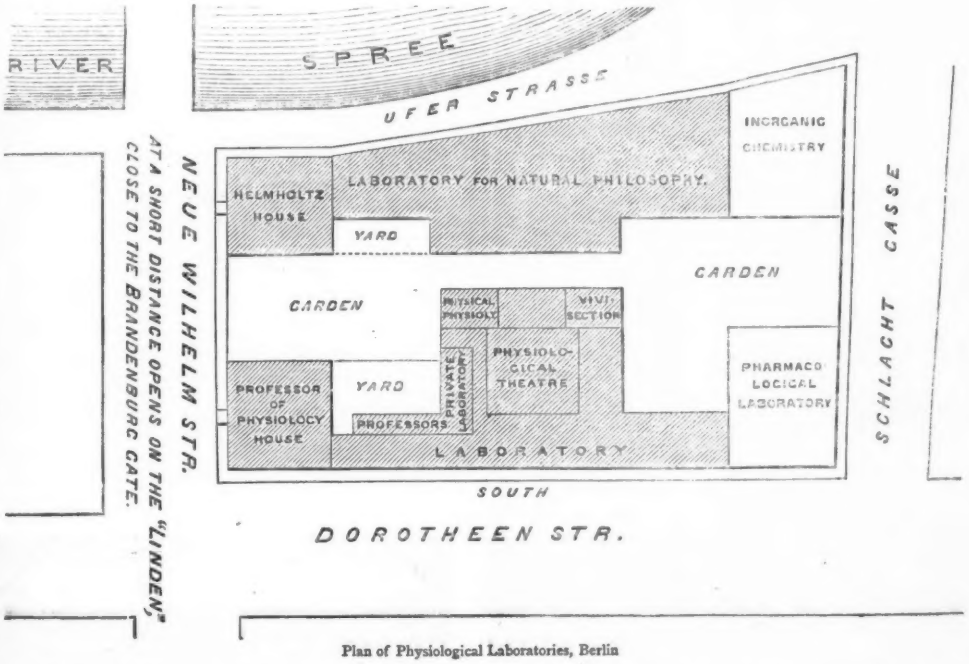
A FRESH survey of the Lower Wealden beds in eastern Sussex by the officers of the Geological Survey Department has quite recently been made. The whole dis-

trict has been recently visited by Messrs. Bristow, Topley, and Drew, and it has been decided to sub-divide the strata hitherto known as the Ashburnham beds into two divisions.

The upper portion, consisting of the mottled clays and shales, will henceforth be called the Fairlight beds, while the lower portion, consisting of shelly limestone intermixed with calcareous shale and *gypsum*, will retain their old title; unless (as is confidently anticipated) they will be found to represent the Purbeck strata, in which case they will be known as the Sussex Purbecks. In reference to our own immediate object, this recent survey has established beyond doubt that the site of the boring is by far the best that the county of Sussex presents for the purpose.

Quite unexpectedly, on January 28, at a depth of 131 feet, a stratified mass of pure white crystalline gypsum (statuary alabaster) was reached. This proved to be over 4 feet in thickness; it was succeeded by 10 feet of gypseous marl; then by 3 feet more of alabaster. Afterwards, we passed through 15 feet of gypsum (more or less impure) varied by seams of crystals of selenite. This discovery has been most opportune. No such accumulation of gypsum was ever met with in Sussex before; and it is some consolation to know that our labour has not been all labour in vain: gypsum is a material which is commercially valuable.

Geologists may therefore inquire, "Where are we now?" The reply is given with caution, and under correction (as the shale seems singularly free from fossils), but as blocks of gypsum are found in the lower strata of the Purbeck series, we assume we are near the base of that formation, and may with some reasonable confidence expect to be able to announce before another quarter is over that we are through these problematical beds, and into the Portland series or some subjacent formation.



Plan of Physiological Laboratories, Berlin

The question of Finance begins to excite some anxiety in the mind of the treasurer. The amount required for machinery, sheddings, &c., has more than doubled the original estimate. Coals, tools, and labour, are each dear, and likely to remain so. The difficulty of access will greatly add to the original estimate of expenses. A large portion of our promised aid is given on conditions which render it unavailable at present.

If 200*l.* could be raised shortly, it would enable the Finance Committee to authorise the call of the second 1,000*l.*; and till this is done we are approaching insolvency. If each existing subscriber would kindly undertake to bring the matter under the notice of some neighbour or friend, we should not only soon raise all we want at present, but be relieved from anxiety for the ultimate prosecution of the enterprise.

We have nothing to do with the commercial value of our present or future discoveries; this will be freely given to those who can utilise it. We can only ask for aid from those who will "give, hoping for nothing again,' except scientific discovery.

THE NEW PHYSIOLOGICAL LABORATORIES AT BERLIN *

THE building of the new laboratory will begin on April 1. The plans are almost ready, and a most glorious place it will be, undoubtedly the finest physiological laboratory as well as the largest which was ever dreamt of. Besides the large theatre, and every possible accommodation for the lectures, it will contain rooms for collections, for a library, a smaller class-room, apartments for three assistants, lodgings for the servant and his family, &c. Then, there are five distinct laboratories most scientifically connected; (1) for physiological chemistry; (2) for physical physiology; (3) for vivisections; (4) for

* Extract from a letter communicated to us by Dr. Bence Jones.

microscopical and embryological investigations. To this laboratory is added a complete aquarium, in which it is hoped to be able to keep all sorts of marine and fresh-water creatures. (5) The private laboratory is organised so as to afford opportunities for every kind of physiological inquiry, so that future professors will feel at home in it, whatever may be their peculiar branch of physiological research. Then, of course, there are dark

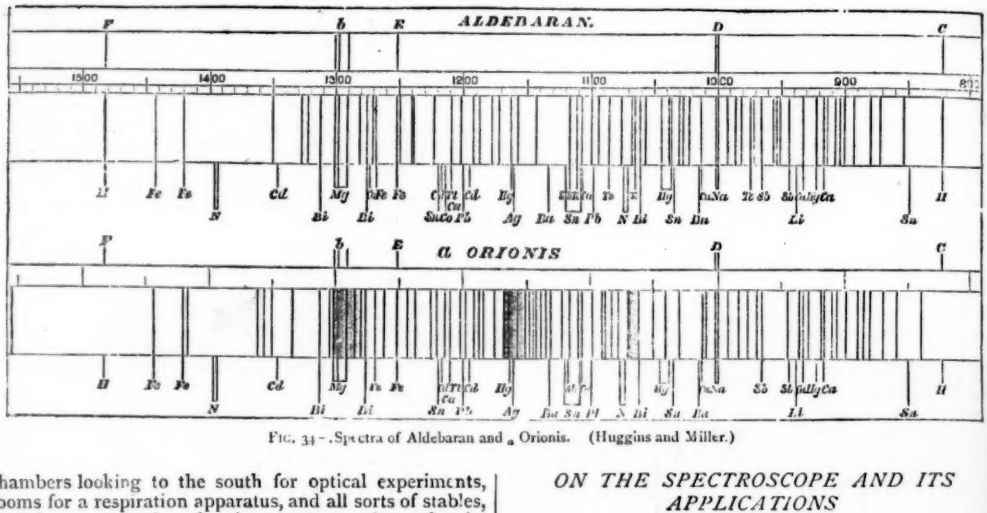


FIG. 34.—Spectra of Aldebaran and α Orionis. (Huggins and Miller.)

chambers looking to the south for optical experiments, rooms for a respiration apparatus, and all sorts of stables, an aviary, a ranarium for the summer, and one for the winter, &c. There is to be a dwelling-house close by, in fact so connected with the laboratory that from the study a lobby and a flight of stairs lead to the private laboratory. The House has been designed entirely according to the English fashion, and wonderful to say, hitherto has not yet met with serious opposition from the architects and the authorities. On the same premises there will be (1) Helmholtz's laboratory and dwelling-house; (2) a laboratory for inorganic chemistry; (3) one for pharmacology, under Leibreich. The accompanying sketch will give an idea of the whole. It covers an area of 4½ acres. The style of building is to be magnificent, much more so than

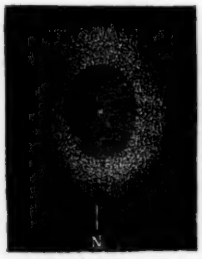


FIG. 35.

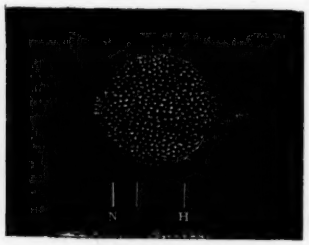


FIG. 36.

Fig 35.—Ring Nebula in Lyra, with its spectrum. Fig. 36.—Planetary Nebula in Aquarius, with its spectrum.

is desirable, because the costliness of the establishment increases the responsibility; but now that they are at it, they do not care for ever so many hundred thousands of dollars. All around the buildings, there will be an area, after the English plan, in order to mitigate the tremor occasioned by vehicles. In the Neue Wilhelmstrasse and the hitherto very nasty lane called Schlachtgasse there remains an open space facing the streets, so that the gardens intervening between the two great masses of building get as much light and air as is possible in the town. After all we are not so exclusively military as it may seem at a distance, and some of the French millions find their way into a scientific channel.

ON THE SPECTROSCOPE AND ITS APPLICATIONS

VI.

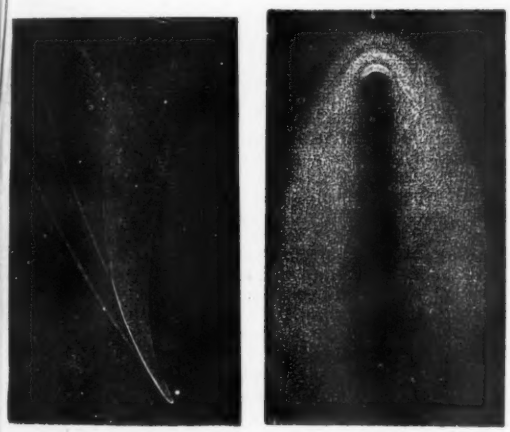
IN the first place, then, what does the spectroscope tell us with regard to the radiation from the sun and the stars? And here I ask you to neglect and banish from your minds for a time any idea of those dark lines in the solar spectrum that I drew your attention to on a former occasion. I hope I shall be able to explain them satisfactorily to you afterwards, but for the present I wish you merely to take the fact that our sun, but for the dark lines, would give us a continuous spectrum. The spectrum of the stars is very much like the spectrum of the sun. In Fig. 34 is seen a representation of the spectra of two stars, α Orionis and Aldebaran, mapped with the minutest care by Dr. Miller and Mr. Huggins.

FIG. 37.—Spectrum of the Nebulæ.—1, 2, 3, lines observed. Above, the solar spectrum is shown from *b* to *F*; below, the bright lines of magnesium, nitrogen, barium, and hydrogen, in the corresponding part of the spectrum.

In both cases we should have a continuous spectrum but for the presence of the dark lines. I think you will see in a moment what I am driving at. Suppose the sun or stars composed of only sodium vapour, for instance, it is clear that their light analysed by the prism would give us no great indication of a continuous spectrum, we should merely get one bright line in the orange. But neglect the dark lines for a moment: dealing merely with the continuous spectrum of the sun and star, it shows that we have a something, whether it be solid or liquid, or whether it be a dense gas or a vapour, competent to give us a continuous spectrum. So we are justified in assuming that sunlight and starlight proceed from the incandescence of

a solid, a liquid or a dense gas or vapour. Again, suppose that instead of looking at the sun or the stars we observe the moon, as Fraünhofer did, as has been before stated, what will happen? We get a second edition of sunlight, in exactly the same way as we should get a second edition of the sunlight in the case of a reflection of it from a mirror; and therefore, if proof of such a thing were needed, the spectroscope is perfectly competent to show us that the moon gives us sunlight second-hand. The same in the main with Jupiter, Venus, Mars, and the other planets. If we study them and observe the dark lines we find that the lines which we observe are generally the same as those which we find in the spectrum of the sun. There are other points to which I shall have to draw your attention on a future occasion, but on the whole, the teaching of the spectroscope is, that all those planets are lit up by sunlight as we know them to be.

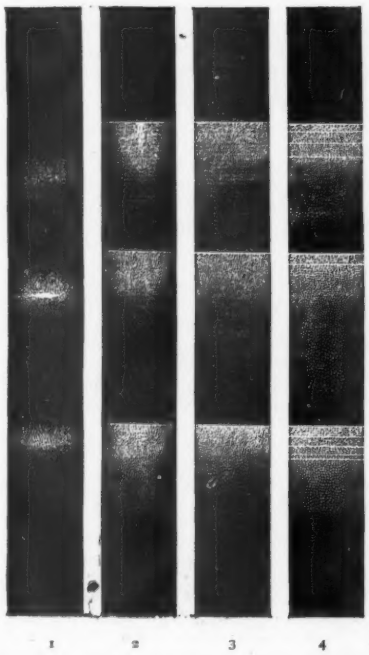
But we have not yet exhausted the wonders of the celestial field; we have dealt merely with the sun and moon, the stars and planets. What about the nebulæ, those strange weird things, dimly shining in the depths of space, both to the eye and in the telescope obviously



General view. Head and envelopes

F.G. 38.—Views of Donati's Comet.

and distinctly different from anything in the shape of the sun or stars? The appearance of these peculiar bodies is sufficient to show us that we have here something very different from the sun or moon. What is it? You all know as well as I do that ever since nebulæ were discovered mankind have wondered at them, and wanted to know what they were; and you are also aware that it was not settled and could not be settled before the advent of the spectroscope, but that it could be settled in five minutes after that event. Mr. Huggins, who first observed the spectrum of a nebula, found that, instead of the continuous spectrum with which you are familiar in the case of the sun and the stars—always asking you to neglect the Fraünhofer lines, which I shall explain afterwards—the light which he got from the nebula consisted merely of three lines. He was exceedingly astonished, so much so that he thought the instrument might be out of order. However, it became perfectly clear to him in a very short time that there was no mistake at all, and that all that the light which came from the nebula could do was to give him these three faint lines. No doubt you have anticipated my explanation. The nebulæ are composed of tenuous gases or vapours. After what I have said about the way in which the spectroscope at once picks out the difference between a solid or liquid, and a vaporous or a gaseous body, you will see at once that these three bright lines indicate that the nebulæ, instead of being composed of solid, liquid, or densely gaseous bodies—instead of being like the sun or stars—are really composed of rare gases or vapours. Mr. Huggins was enabled, in fact, to determine the gas in one instance, for one of the lines he found was coincident with one of the principal lines in the spectrum of hydrogen one of the other lines possibly being due to nitrogen. And now comes another extremely important point, showing the importance of studying the most minute changes in gaseous spectra, for Mr. Huggins, who knew the spectrum of hydrogen and the spectrum of nitrogen well, and who knew how extremely complicated those spectra are at times, was much astonished at finding only one line of hydrogen and one of nitrogen, and attempted to account for the singleness of the lines, first, by assuming a condition of the gas different from anything



1 2 3 4

FIG. 39.—1, Spectrum of Brorsen's Comet; 2, Spectrum of Winnecke's Comet; 3, Spectrum of Carbon in olefiant gas; 4, Spectrum of Carbon in olive oil. (Huggins.)

we meet with in our laboratories, and again by assuming an absorbing medium in space. But after Dr. Frankland and myself had made some observations on the spectra of hydrogen and nitrogen, we found it was perfectly easy to obtain, and sometimes when one did not want it, a spectrum of hydrogen or of nitrogen giving only one line, or nearly so; so that by comparing the conditions which were necessary to obtain these conditions in our tubes with the conditions of the nebulæ, it was quite possible to make at all events a rough guess at what is the constitution of the nebulæ, so far as pressure or molecular separation goes. We find, for instance, this single line of hydrogen, and a nearly single line of nitrogen, when the pressure is so slight that you would say that the tube really contained nothing at all, and when, moreover, the temperature is comparatively low. Now, not only is this a fact, which we are quite prepared to assert, merely on the evidence rendered us by these tubes, but I think you will acknowledge that it is entirely in accordance with everything we know astronomically on this subject.

For the next application of the spectroscope in this direction, let us take a comet. The appearance of a

comet is probably well known to many, who will recollect the form of Donati's comet. Although, as you know, that comet appeared only about ten years ago, unfortunately it came too early for us to learn anything about it by means of the spectroscope. We have, first of all, an extremely bright nucleus; then a kind of semilune of greater brilliancy than the rest of the head, then what is called the coma, and the tail. The question which the spectroscope had to put to the comet was—of what is the nucleus composed, and of what is the tail composed. Prof. Donati, and Mr. Huggins especially, to whom we owe so much for his work in this direction, has made some observations on two small comets—I am sorry they were not larger—with considerable success. He found that in the comets he examined, the head gave out a light which very strongly indeed resembled the spectrum of carbon vapour. The spectrum of carbon taken with the spark in olive oil and in olefiant gas differs slightly; the spectrum as obtained from the latter consists of three bands or waves of light, which commence tolerably bright and sharply on the red side, and become gradually fainter towards the more refrangible side. These bands are severally situated in the beginning of the green, in the true green, and in the blue portions of the spectrum. Mr. Huggins has also observed the spectrum of Encke's comet, and has confirmed the result that he previously obtained, viz., that the spectrum of the comet is identical with the spectrum of carbon, as taken in a hydrocarbon. I should like to draw your attention, if there were time, to the way in which these spectra of the carbon spark taken in oil and in olefiant gas, differ.

I have not yet completed all I have to say on the subject of radiation. If, as we have already seen, we take a tube containing incandescent hydrogen and pass a series of intense electric sparks through it, we see that it gives out a red light, which may remind you of some other specimens of radiation which is supplied us by the skies. I allude to the red prominences which are seen around the sun, not in ordinary times, but when the sun is eclipsed. This representation gives you a good idea of what really is seen when the sun is eclipsed, when we have as it were a black sun instead of a bright one, which is really nothing, but the body of the moon. Around this we have a ring of light, which is called the corona, and here and there in this corona we have what are called red flames and red prominences. These red prominences have also on closer observation been found to be only local aggregations or heapings up of a red layer which surrounds the outer edge of the sun. Here, then, it was quite possible that if the newly invented spectroscope were set to question these things, we should see at once whether they were solid or liquid, or whether they were gaseous or vaporous. If we got a continuous spectrum from these red things, we should know that they were solid, or liquid, or densely gaseous. If, on the contrary, we got a bright line spectrum we should know we were dealing with a gas or vapour. You also see that, as the light is red, the chances were that they were not solid or liquid, and then you further see that if the things do consist of a light which does give us lines, a determination of the exact position of the lines, and a comparison of these positions with those of hydrogen, sodium, magnesium, barium, or anything else, would teach us what these things were.

J. NORMAN LOCKYER

PROF. FLOWER'S HUNTERIAN LECTURES

LECTURES XIII. XIV. XV.

TAPIRIDÆ. The geographical distribution of the existing members of this small order is very peculiar, they being confined to the Malay Peninsula, Sumatra, and most of South America. Lund has found their remains in the Post-pleistocene caves of Brazil; they have also been obtained in abundance from similar deposits in North America, and these can hardly be distinguished from those at present existing; in China likewise Pleistocene Tapir's teeth have been found. In Europe during the same time they do not seem to have existed, although Elephants and Rhinoceroses were abundant. In the Pliocene and Miocene, Tapirs are not unfrequently met with at Eppelsheim, Auvergne, and elsewhere; perhaps they originated in Europe, and thence spread east into Asia, and on to America. Respecting their anatomical peculiarities, the teeth are forty-two in number, the anterior lower premolar being absent; the molars and premolars are much alike, forming a uniform series; the incisors are smaller than the canines, they have a small cingulum. The molars are a modification of those of Lophiodon, the transverse ridges are very prominent, and the cusp of the cingulum is less developed. The lower possess two simple transverse ridges, as in Lophiodon, but the last in the series wants the extra back lobe. The anterior nares are very open and the orbit is incomplete behind. There are four toes on the front foot, and three behind; the radius and ulna as well as the tibia and fibia are quite separate and well developed; *T. bairdi* is peculiar in that the meseth-moid cartilage is well ossified, and the maxillaries are specially developed upwards to support it.

The *Palæotheridæ* occur in the Upper Eocene only, they were first found at Montmartre and worked out by Cuvier; since that time they have been obtained from many parts of France, the Bembridge clay, near Yarmouth, in the Isle of Wight, and in Hampshire. Several genera have been separated off, and about a dozen species, from the size of a small rhinoceros downwards. In general aspect they must have been tapir-like. The maxilla curved downwards in front as in the tapirs; the orbital and temporal fossæ were also united, and there were large anterior osseous nares; the feet were much like those of the tapir, though they were more specialised in wanting the fifth toe to the manus. The typical forty-four teeth were present; the incisors were more uniform than in the tapirs; the first pre-molar was rather rudimentary, the others formed a uniform series with the molars, which were wider than from before backwards, much pressed together, and with short crowns. They can be shown to have been developed on the type of Lophiodon, the outer wall bulging inwards, opposite the outer cusps, instead of outwards, giving the earliest indication of the lunate type of tooth; the transverse ridges were normal, and the internal cusps were slightly cut off from them, turning backwards as the rudiments of the posterior semilunes. The lower teeth presented a peculiarity here first noticed, each being formed by a double crescent, quite different from those of the tapir. The last lower molar had a third crescent behind as in Lophiodon and the Artiodactylata, but, different from the latter, in the corresponding milk tooth not presenting it. *Palaplotherium* was a smaller and earlier genus described by Owen from Hordle. In the upper jaw the first premolar was missing, and the corresponding lower one soon lost; the others were comparatively simple. The remains are very abundant, the feet were as in Palæotherium. Gervais has given the name *Propalæotherium* to a few teeth of another early form, intermediate between Lophiodon and Palæotherium. *Anchitherium* was an American form closely allied to the strictly European Palæotheridæ.

Rhinocerotidæ are at present found in Africa and South Asia only; they belong to three types, the African two-horned, non-scutellated; the Asiatic two-horned, and the Asiatic single-horned. The extinct members were numerous; four species existed in England. They did not appear before the Miocene epoch; many are found in America, but not above the Pliocene period. The existing genera have peculiarities in their incisor dentition; these teeth are quite absent in the African, and two above as well as below in the Indian species; when they are

present the outer upper and the inner lower are rudimentary. The canines are absent in all; the full complement of molars are present, of similar character, and degenerating at either end; they are formed on the Lophiodon type; the outer wall is very strong and oblique, with the cusps but little developed and the cingulum large behind; the posterior transverse ridge sends forward a process from near its middle, which in one fossil species (*R. tichorhinus*) is met by another from the anterior wall to form a circular foramen behind the anterior fossa. The lower molars agree with those of Palæotherium, being formed of a double crescent, in which the posterior cornu of the front lunule is partially overlapped by the anterior cornu of the hind one; no third crescent is found on the last molar; three toes are present on all the limbs. They have not been found fossil in the Eocene strata, consequently the American species are among the earliest. Leidy has named an allied genus of small size *Hyracodon*. Its teeth resembled rhinoceros, but the anterior premolars were retained; the peculiar *uncus* on the posterior transverse ridge was wanting, and the proportions of the incisors were reversed. There are several extinct species of the genus Rhinoceros. *Acerotherium* possessed the same number of teeth as the Asiatic genera, but the nasal bones were small, slender, and smooth above, so they could scarcely have carried a horn; it is a Miocene form only; a fifth rudimentary toe was present. Except *R. pleuroceros*, which had two laterally-placed tubercles on the nasal bones, all the other species had them median. They may be divided by their incisors, as are the recent genera, some having them rudimentary, others not. All the European specimens had two horns, with or without functional incisors. The English species, which are not peculiar, are from Pliocene and Pleistocene formations; in *R. leptorhinus* the nasal septum was not ossified; in others it was much so, as in *R. tichorhinus*, a species which has been found preserved by ice in the river Vilni, a branch of the Lena, in Siberia; it possessed a hairy coat and the peculiar pit in the molars mentioned above.

From Port St. Julian, in Patagonia, Mr. Darwin first obtained bones of the peculiar genus *Macrauchenia*, which has not been found out of South America, and only in the Pleistocene deposits there. Prof. Huxley has proved the existence of a second smaller species from some fragments out of a copper mine in Bolivia. Owen showed in his, the first description of the animal, that the vertebræ were peculiar, and agreed with those of the Camels in having the vertebral artery threading a bony canal inside the spinal column, instead of through the bases of the transverse processes. It may be remarked that Myrmecophaga exhibits a similar conformation. But these vertebræ in Macrauchenia are further peculiar in having both ends of the centra quite flat instead of their being opisthocœlous, as in the allied forms. M. Bravard, who was killed in the earthquake at Mendoza, left excellent drawings of the skull and other parts of this animal, which Prof. Burmeister has since published. From them we learn that the skull was not unhorselike; the orbital ring was complete; the palate was not fully ossified between the posterior molars (the camels present the same peculiarity, though Artiodactylate); the nasal bones were extremely reduced, so that the anterior nares were directly above the posterior, and the lower jaw had the angle prolonged. Burmeister thinks, and with good reason, that the animal possessed a fair-sized trunk. There were twenty-four dorso-lumbar vertebræ, of which seventeen were dorsal. The radius and ulna, as well as the tibia and fibula, were fused throughout. The femur possessed an extremely small third trochanter; and there were three toes to each limb. The astragalus was strongly Perissodactylate, no cuboid facet being present. Our knowledge of the teeth is somewhat deficient, as they are always preserved in a much worn state. The typical forty-four were present; the incisors were equine, and the canines of the same size; the back molars were the bigger and the anterior premolars comparatively simple. The lower molars formed double crescents, as in Palæotherium.

In tracing back the descent of the *Equidæ*, the Palæotherium d'Orleans of Cuvier has been shown to be generically different, and has been called *Anchitherium*; it is also found in Nebraska. These were small horse-like animals with teeth much as in Palæotherium, forty-four in number; the first premolars were very small, and no pit was present in the incisors; the outer wall of each molar was also concave opposite the cusps; the lower molars formed double crescents, and the last possessed the extra lobe. The ulna and fibula were fused with the radius and tibia respectively; the astragalus had some of the obliqueness of that of the horse, which it resembled in many other points. But there were three toes on the limbs, the lateral ones being less strong than the median. A peculiar antorbital fossa was present.

The horse must be described before the affinities of its close allies can be realised. In it the incisors possess the well-known pit; the canines are rudimentary in the mare; the premolars resemble the molars, and the crowns are very long and deeply embedded, with a concave crescent opposite the tubercles on the outer wall and the anterior internal tubercle insulated at first; otherwise they are typical. The depressions are very deep and are filled up by cementum, to form a solid mass. The lower molars are slightly complicated double crescents. The ulna and fibula are not free. *Hipparion* had very horse-like teeth. It is a later Miocene form, and is common in the New and Old World. It possessed the antorbital pit, as in Anchitherium, but was otherwise very equine. The canines were present in both sexes of equal size, and the anterior internal tubercle of the molars was completely insulated. The median of the three digits alone was functional. *Merychippus*, a Pliocene form, recognised by Leidy from some teeth, seems to have been an intermediate form between these and Anchitherium. Fossil true horses abound in America as well as the Old World; they since became extinct in the former locality. They are found in the Pleistocene nearly everywhere; their earliest remains are from the Sevalik Hills.

With these animals the description of the fossil Perissodactylata terminates.

PERCEPTION IN THE LOWER ANIMALS

LETTERS on this interesting subject still continue to pour in upon us in so great abundance that limited space compels us to select merely the facts contained in each. The best service we can at present render to the unravelling of the, we think, yet unsolved problem is simply to accumulate facts; no doubt a satisfactory explanation will by-and-by be arrived at. First we must give place to Prof. Croom Robertson, who thus writes as to the theory broached in his former letter:—

In my former letter I made no pretension to explain all the wonderful feats reported of dogs or other animals, but only argued, in the wake of Mr. Wallace, that it had never been sufficiently considered what help in finding their way dogs might have from smell alone. Be the help what it may in the particular cases, I thought it clear that, if in their common experience smell does not somehow supply to dogs the defect of touch, they are, as far as we can see, badly fitted out, by comparison with men, for making their way through the world. And, even after your article of last week, I must still in their interest hope that the notion of a continuous world of smells is not an impossible one.

If the external world were the same to dogs that it is to men—a complex of interwoven touches and sights in space, and only in addition dogs had more frequent and varied experiences of smell, the dying away or shifting of some in a particular train of odours would doubtless, as the writer of the article urges, put a dog out

when reduced to work its way back along such a train. But my point is, that a dog will regularly think of all things, stationary or moving, by their smell, where we think of them by their touch as handled, and this upon the simple ground of fact that a dog has no hands; in which case the continuity of a road will as little to the dog as to us depend upon the standing-still of flocks of sheep or any other passing objects. It is true that our experience does not enable us easily to fancy what sort of world this of the dog's will be, but at the worst we need not conceive it, with the writer, upon the analogy of a succession of coloured mists. Do we, even with our intermittent smell, find it so impossible to refer the diffused odour of a dung-hill to a particular source? Or, to take a fair parallel case, if a sound is diffused so that it may be heard anywhere throughout a great hall, do we therefore suppose it to be everywhere and not to emerge from a definite spot? To the psychologist the strictly tactile properties of objects are themselves but sensations, which we are determined to project away from us in a certain definite order—as it happens, a very sharply defined order. With different means of projection and different sensations to project, how should the dog not have its own different world—the best it can devise out of its experiences?

Such reference to the fact of a dog's organs of sense being what they obviously are, ought not to be discounted as mere speculation, but perhaps that must be borne with. Facts of the other sort—reports, more or less authentic, of the feats of particular dogs—when made a ground for ascribing to the species preternatural powers of divination, merely because the facts are not explicable under the conditions of human experience, are beset with their own difficulty. Dogs do not always find their way back, even from the next street. Let all that side of the matter be thought of, before we suppose some unerring instinct to account for the remarkable enough feats of some that cannot be denied. Of course no train of evanescent smells can guide a dog back upon a road from which they have died away; as little, or still less, can the succession of particular smells, however constant, lead a dog right upon a line that he has never travelled over before. But that dogs, while they have no such touch as ours, do constantly use their sense of smell to guide them, cannot be doubted; and the result to them must be such a very different world of experience from ours, though developed under common laws of acquisition, that we have no means of deciding what is impossible to be done by *some* dogs through mere experience.

One of your correspondents, Mr. Brewer, had good remarks in this sense the other week. One point he raised besides upon which I would add a word. The point was whether for the dog smells would enter, instead of touches, into that fundamental experience of an external world, of which visual sensations are but marks or symbols. I should imagine that they would enter into its experience of modes of extension, by us acquired chiefly through the moving hand. But into the experience of modes of resistance, the general tactile sensibility diffused over the surface of the body would enter for the dog as well as for us.

Mr. George Henry Lewes appends to his letter on p. 401 of this week's NATURE, the following contribution to this subject:—

Gratiolet, in his work on the "Nervous System," mentions that a dog of his was always thrown into convulsions of terror by the scent of a small piece of wolf's skin, which was so old that it was worn to a shred. In my room there is a perfectly unworn wolf-skin made into a rug, and on this my bulldog was accustomed luxuriously to stretch himself, without any but pleasurable emotions. Now this may have been due either to his imperfect sense of smell, or, what is more probable, to his not having inherited any terror from ancestors more likely to attack than to be afraid of a wolf.

Mr. Laughton, of the Royal Naval College, Greenwich, sends us a valuable letter, from which we extract the following:—

A passage in Sir Bartle Frere's paper on Cutch (Journal of the Royal Geographical Society, vol. xl. p. 186), seems to bear on this subject which has been interesting the readers of NATURE for some weeks past. He says:—"As elsewhere in the plain country of Sind, and here more conspicuously, owing to the absence of any prominent natural features or marked tracts, the best guides seem to depend entirely on a kind of instinct—they will generally indicate the exact bearing of a distant point which is not in sight quite as accurately as a common compass would give it to one who knew the true bearing. They affect no mysterious knowledge, but are generally quite unable to give any reason for their conclusion, which seems the result of an instinct—like that of dogs and horses and other animals—unerring, but not founded on any process of reasoning, which others can trace or follow."

I incline strongly to the solution put forward by the writer in the *Quarterly* (see letter in last number). If to this we add the consideration that dogs certainly can and do interchange ideas, and may therefore question other dogs as to the general direction in which they wish to go, the two together seem to offer a reasonable though hypothetical explanation of the very curious facts referred to.

Mr. George R. Jebb, of Shrublands, Chester, writing on March 18, says—

Last Thursday I sent my terrier dog (Tartar) by train from Chester to Shrewsbury by Great Western Railway (*i.e.* by way of Wrexham and Ruabon), I myself went by the North Western line *viâ* Broxton and Whitchurch; the distance by the former road is 42 miles, by the latter 38: the two railways diverge from each other for some 20 miles from Chester, and are then 16 miles apart; they afterwards converge and join at Shrewsbury. Tartar was sent from Shrewsbury to Broxton station, which is 10 miles from Chester, by the 2.55 train. I had previously arranged with the station-master to keep the dog for five or ten minutes after the departure of the train, and then to set him at liberty on the public road. The train arrived at Broxton at four o'clock. Tartar hung about the station till nearly 5.30, perhaps longer, as he was not seen starting off. He was at home at Chester at nine; he was not at all distressed. It is probable, I think, that he came back pretty direct. It is certain he came across ten miles of country, the greatest part of which he had never traversed before. It is also certain he did not return *viâ* Shrewsbury, as there was not time. He had never been at Broxton before.

Does not this experiment seem to prove that dogs—some at least—possess the wonderful power (the nature of which is at present unknown) of arriving at the knowledge of the *direction* of their home when they have been taken from it long distances by circuitous routes? And if so, is it not more probable that a dog when lost usually makes use of this power to guide himself home by the shortest practicable road, than that he finds his way back "by means of the odours he took note of" on the outward journey? How do pigeons find their way home? A railway contractor told me he has a pony which he uses chiefly for drawing a light "lorry" upon a tramway now in course of construction. There are on this tramway some loops or passing places for waggons at intervals of a mile or so. These points are dangerous if passed too quickly. The contractor drives the pony himself often at a very fast rate. The pony will on the darkest nights suddenly pull up at the dangerous points without the slightest check from the driver, who otherwise would be obliged to proceed with the greatest caution. Does the pony know his whereabouts by the sense of smell, hearing, or touch? Probably, I should say, by all three acting in unison.

The Rev. O. Fisher, of Harlton, writes:—

On a bright day when I have flowers in my window, bees frequently precipitate themselves against the window-panes, evidently desirous of reaching the plants. This is easily explained by the sense of sight; but the remarkable thing is that they do the same when the blind is drawn down, so that they cannot see the flowers, and it seems impossible that they should smell them while the window is shut. Can it be that that all-pervading æther, which brings light to our eyes, and is also believed to convey the magnetic and electric forces through media impervious to light, may act in a manner other than luminiferous towards some animals, and produce "action at a distance" upon their organs?

A Scotch correspondent, R. C., who has given us his full name and address, sends us the following interesting facts:—

A few years ago a sheep, one of a flock, belonging to Mr. Miller, flesher, Beith, Ayrshire, gave birth to three lambs; thinking that three were too heavy for the mother to suckle, he gave one to a farmer, who lived three quarters of a mile from the field where the sheep lambed. This one was taken away from the mother when barely a day old, and carried to the farmer's, where it was shut up in a close house.—

Two days after it found its way out, the door having been left open, and immediately made off for the field from which it had been taken. The writer met it near the field walking (it having chosen rightly between two branches into which the road diverges between the field and the farm-house) on the grass on the side of the road: the farmer, an old man, was in pursuit, and called upon the writer to turn it back, which he did, but not without some difficulty, as it crossed from side to side, and made efforts to pass on, with all the tactics of an old animal, while it was barely three days old, and had been barely one when it was brought from the field.—I have the following from a well-authenticated source. A farmer in Bogside, Beith, of the name of Fleming, was looking out of his window one summer's morning, about three o'clock, when he saw a fox crossing a field before it, carrying a large duck that he had captured. On coming to a stone dyke about four feet high, on the side of the field, Reynard made an effort to leap over it with his prey, but failed, and fell back into the field. After making three attempts with the same result, he sat down and viewed the dyke for a few minutes; after apparently satisfying himself, he caught the duck by the head, and standing up against the dyke with his fore paws, as high as he could reach, he placed the bill of the duck in a crevice in the wall; then springing upon the top, he reached down, and pulling up the duck dropped it upon the other side, leaped down, and picking it up, went on his way. If this is not reason, it is nearly akin to it.

We conclude with the following instances sent us by Mr. G. J. Romanes, of Cornwall Terrace, Regent's Park:—

A Colley dog accompanied his master with a flock of sheep down the Caledonian Canal, and between Oban and Greenock suffered much from sea-sickness. Several months afterwards a similar journey was undertaken by the same dog and man with another flock of sheep. Upon quitting the wharf at Oban, the dog, remembering that this was the point at which his troubles began on the former occasion, jumped ashore, leaving his master with the sheep on board the steamer. Upon landing at Greenock the man was surprised to find his dog upon the quay awaiting his arrival—the animal having run by land from one wharf to the other, over ground which he had never before traversed. The distance between Oban and Greenock is fifty miles in a straight line, but as this passes over high mountains as well as through a lake and two arms of the sea, it is not likely to have been the route taken.

My authority for this account is a leading clergyman in Glasgow, who would, no doubt, be willing to give his name to any one desiring it.

The second instance, in its bearing upon Mr. Wallace's theory, is even more conclusive. Another dog of the same kind sailed with his master from Wick to Berwick, where he was lost. Ten (?) days afterwards he appeared at his home in Sutherlandshire, footsore and exhausted, having, it must seem, run nearly the entire length of Scotland. I am indebted for this information to a medical army-officer and well-known C.B. who had heard it from the owner of the dog. As my friend is at present in ill-health, I am unable to refresh my memory as to the number of days occupied by the dog's return journey, but I think it is correctly stated.

NOTES

The meeting to which we alluded last week in connection with a memorial to the late Prof. Sedgwick, was held on Tuesday, and was attended by a large number of scientific and university friends of the late eminent geologist. Resolutions were passed that a geological museum be erected, to be called the Sedgwick Museum, and that a bust of the professor should be placed in it. A Cambridge and a London Committee were appointed. The Prince of Wales wrote that the object of the meeting would have his warm support, from the feeling of respect he entertained for the late professor.

Some efforts are now being made gradually to give the same stimulus to the higher education of women as of men. The National Union for Improving the Education of Women has offered seven scholarships of 25*l.* each, tenable for one year, for competition throughout the United Kingdom, the competitors to be young women over sixteen years of age. The scholarships will be awarded at the local examinations held during the present year by the Universities of Oxford, Cambridge, and Edinburgh, and Trinity College, Dublin, the Science and Art Department, the Society of Arts, and the College of Preceptors.

The examiners for the Cambridge Natural Sciences Tripos for 1872 have represented to the Board of Natural Science Studies that they are of opinion that the time has now come when an increase in the number of examiners is urgently required. The amount of physics now included in the subjects of examination is so large as to make it impossible to treat the examination in this subject any longer as an appendage to the examination in chemistry. The subjects of comparative anatomy, zoology, and physiology are also too wide to be undertaken as a general rule by one examiner. An increase in the number of examiners to seven would make it much more often possible to secure a real examination of the answers by two examiners, which is unquestionably desirable. The Board therefore recommend that in Regulation 10 for the Natural Sciences Tripos, for the words "two examiners" the words "three examiners" be substituted, and for the words "third examiner" the words "fourth examiner" be substituted, and for the words "five examiners" the words "seven examiners" be substituted. They recommend further that, in order to ensure the regular rotation of examiners, five examiners be nominated by the Board in the present year, of whom one shall be nominated to hold office for one year only.

Mr. Arthur Milman, son of the late Dean of St. Paul's, has been appointed Assistant-Registrar to the University of London, in the room of Dr. Hirst.

It is understood that Mr. Fowler, of Lincoln College, Oxford, author of the two works on Deductive and Inductive Logic, will be a candidate for the Professorship of Logic, vacant by the recent death of Prof. Wall. The appointment is made by Convocation.

The late Mr. Julius Brenchley, whose death a month ago has been a great loss to scientific collectors, as well as to the town of Maidstone, left as the results of his voyage in the South Pacific, the last of his most extensive travels, the manuscript together with the plates which illustrate it already drawn, of a work which he fully intended to have had printed, on the natural history of those regions. It is to be hoped that some means will be taken to insure their publication.

It is satisfactory to find that that the new "Spanish Society of Natural History" is continuing its career undisturbed by the political troubles around it. The third part of its Annals bearing date March 5, 1873, has reached this country, and is quite up to the mark of the parts which have preceded it. It contains the conclusion of Vilanova's paper on "the Pre-historic in Spain;" a catalogue, by Gundlach, of the mammifera of Cuba; a paper by Sharp, describing a new species of Spanish Coleoptera, and containing the descriptions of several new blind beetles from the caves of the mountains of the Asturias; a paper, by Colmeiro, on the elevations attained by cultivated plants in Ecuador; also a long and careful paper by Colmeiro, on the Leguminosæ of Spain and Portugal. This part completes the first volume, and contains index, and list of the members of the Society. On inspecting the latter it appears that only two of our countrymen have joined the Society. This fact has, we believe, been a considerable disappointment to the founders of the Society, who hoped it would meet with a liberal support in this country. We hope that when the existence of the Society and the merit of its publications become more widely known, it will receive the recognition it deserves.

THE brothers Godeffroy, large merchants of Hamburg, through the instrumentality of several collectors in the Pacific Islands, have accumulated a large number of specimens of marine and other animals, many of great rarity. They have lately placed their material in the hands of naturalists who interest themselves in the different departments, and their results are being published in the *Journal des Museum Godeffroy*, a quarto work, the first part of which, excellently illustrated, has just appeared.

PROF. MAX MULLER delivered his first lecture on Mr. Darwin's Philosophy of Language at the Royal Institution last Saturday; the other two will be given on Saturday next and Saturday week. From the syllabus which is before us, these lectures are likely to be of high value, and to throw much light on the subject under discussion, and in general on the place of man with reference to the lower animals. No doubt the lectures will be given to the world in a more permanent form after their delivery at the Royal Institution.

THE Rev. Mr. Moyle, lately sentenced to penal servitude, is not, as stated in the newspapers, a Fellow of the Royal Society.

THE Geologists' Association have arranged the following visits for March and April:—Thursday, March 27—Visit to the British Museum, at 3 P.M., to inspect those portions of the Botanical Collection interesting to geologists. Thursday, April 3—Visit to the Museum of the Royal College of Surgeons, at 3 P.M., to inspect the Hunterian and Zoological Collections preserved in the Museum of the College. Easter Monday and Tuesday, April 14 and 15—Excursion to Banbury, Oxfordshire, assembling at the Red Lion Hotel, Banbury at 12.30 P.M. Monday, April 21—Visit to the Museum of Practical Geology, at 8 P.M. under the guidance of Mr. Etheridge, to inspect the Palæontological Collections exhibited in the Galleries of the Museum. Saturday, April 26—Excursion to Charlton from Charing Cross by the 2.52 P.M. North Kent.

THE second annual meeting of the Glasgow Society of Field Naturalists was held on the evening of Tuesday the 18th inst., Mr. J. Allan, vice-president, in the chair. The report read by the Secretary showed that a considerable amount of work had been done during the year. Twelve excursions were held to places of interest in the neighbourhood. The papers read were numerous, varied, and interesting, and a large number of specimens were exhibited. The branches to which more particular attention was given were botany, entomology, and marine zoology.

THE largest catalogue of stars that has ever been published in America is now about to appear from the United States Naval Observatory at Washington. This work, as we learn from a recent communication of Prof. Yarnall, will embody all the valuable observations made since the foundation of the observatory, in 1842, with the meridian instruments, consisting of the work of the well-known astronomers, Coffin, Hubbard, Ferguson, Newcomb, Hall, Harkness, and Yarnall. Over fifteen years of labour have been devoted to it by Prof. Yarnall and his assistants, and he has himself made nearly one half of the observations. The catalogue will be based on over eighty thousand observations of more than ten thousand stars, many of them being quite faint, and in extreme southern latitudes, such as have never, or rarely, hitherto been observed.

BY the publication of a supplementary number, containing the proposed corrections of plates already issued, the important work of Mr. William H. Edwards upon the butterflies of North America, completes its first volume. No American work of the kind has ever been printed containing in its pages so satisfactory illustrations of the various species, new and old, as this of Mr. Edwards. The volume, as finished, embraces fifty plates, each containing several figures, representing all the varieties of each species.

AT the annual meeting of the Royal Irish Academy, held on Saturday evening, the 15th inst., the Cunningham Gold Medal was presented to Sir William Robert Wilde, Knt., M.D., in recognition of his valuable services in the compilation of the Museum catalogue, and in the arrangement of the Museum.

THE American Palestine Exploration Society has reached Syria, under the command of Lieut. Steever, United States Cavalry, accompanied by Prof. Paine, formerly of Robert College, Constantinople, and by other persons, and at last advices was fitting out at Beyrout, with a view of taking the field early in March. An arrangement has been made with the British Palestine Exploration Society by which the whole country east of the Jordan, and embracing the old territories of Moab, Gilead, and Bashan, are to be relinquished exclusively to the American society, and it is expected that, abounding as it does with ancient ruins and excavations, objects of much interest will be brought to light.

AMONG other bills lately presented to Congress is one for the establishment of a National Photographic Institute, which provides for the establishment of such an organisation in Philadelphia, where the entire theory and practice of the photographic art are to be taught by competent professors, under the direction of the National Photographic Association of the United States. The bill also provides that the sum of 30,000 dollars shall be appropriated for the purchase of a suitable building and apparatus, but that the institution shall be self-supporting, and only such fees shall be paid by the students as shall meet the actual expenses.

WE have received a couple of Salem (Massachusetts) papers, containing detailed accounts of the celebration on March 5 of the 25th anniversary of the Essex Institute of that city. This institute, mainly scientific in its aims, can trace its origin under various forms to about the middle of the last century, and under its present name was constituted by the union in 1848 of the Essex Historical and the Essex County Natural History Societies. Prof. O. C. Marsh, of Yale College, who was present, spoke of the good work which the institution has done in diffusing scientific knowledge and encouraging other societies; he also acknowledged that it was at the hands of this institution that he acquired his first taste for scientific investigation.

MR. PARTRIDGE, for many years Professor of Anatomy to the Royal Academy, died on Tuesday, 25th inst.

The following telegram from Mr. Cowie, Shanghai, dated March 25, $4^h\ 5^m$, has been received by Mr. J. R. Hind:—"Your predicted circular black spot on sun, seen here distinctly at 9 morning, 24th." This of course refers to the possible transit over the sun's disc of an intra-Mercurial planet, and although it is very unlikely that Mr. Cowie's is a genuine find, the mere fact that he should put himself to the trouble and expense of sending such a telegram all the way from Shanghai, is an encouraging sign of the increasing and wide-spread interest taken in science.

AN International Congress is to meet in Vienna on August 4, to discuss the question of Patent Rights. The Congress, which was suggested by President Grant, will consist of scientific men, manufacturers, political economists, and skilled workmen. Each Government will be represented by a special delegate.

THE *Practical Magazine* has now reached its third number, and so far has carried out satisfactorily the promise of its prospectus; its main aim being to carry out a careful and systematic survey of the Industrial Activities of America, Germany, and France, in order to present at the earliest possible moment such information as is likely to be useful to British practical men. We believe there was a place in Britain for such a journal, and if the *Practical Magazine* continues as it has begun, we have no doubt it will satisfactorily fill this place. In get-up, paper, printing, illustrations, &c., it is one of the handsomest journals

we have seen, and we hope it will have many readers both among industrial employers and employées.

Two attempts have recently been made from Norway to reach Spitzbergen in the middle of winter, for the purpose of taking additional supplies to the storehouse at Eisfiord, erected and fitted with all necessaries last summer, as we noted some months ago, for the purpose of sheltering the exploring expeditions which are endeavouring to penetrate polewards to the north of Europe. The steamer *Albert* left Tromsoe on November 20, and reached about 77° under the meridian of Greenwich, when, on account of the great danger from the ice, not to mention the unbroken twilight, and the improbability of reaching the goal, it was determined to put back. One result of the voyage is the observation that the temperature of the sea at that season is several degrees higher than that of the air. In spite of the failure of the *Albert*, the sailing-vessel *Isbjärn* left Tromsoe on December 24, with the same object in view, and came within sight of Bear Island on January 7, which, however, it was found impossible to reach. After one or two attempts in other directions, the *Isbjärn* was compelled to put about, more from the difficulty of managing the frozen sails than from the danger from ice and the inconvenience of perpetual darkness. Notwithstanding these two failures, we learn from *Les Mondes* that M. Rosenthal, of Bremen, has fitted out his steamer *Groënland* for another attempt. M. Rosenthal has already lent his vessels to the service of science, and we hope this third attempt may be more successful than the previous ones, though it seems hopeless.

An advanced sheet sent us of Petermann's *Mittheilungen* contains an article on King Karl Land, the island which lies to the east of Spitzbergen. English geographers identify this island with Wiche Land discovered by the Englishman Edge, in 1617, while Prof. Mohn, the writer of the article referred to, claims it for the Norwegian discoveries of 1872, and names it King Karl Land, after King Karl XV., of Norway and Sweden. Dr. Petermann maintains that Wiche Land has no existence, as the position given to it until recently in the maps was considerably south of King Karl Land, where there is nothing but water. Dr. Petermann in a note to us suggests that if the English Admiralty or any private English expedition should explore and survey it thoroughly, there might be no objection to naming it afresh. The naming of any geographical discovery is not of very great importance, but it seems to us that the discovery of the island really belongs to Edge; all that can be said against it is that either he or subsequent geographers misplaced the island by a few degrees. On the same ground the credit of many early discoveries might be taken away from those to whom it is justly attributed.

It is said that an American aëronaut, Prof. Donaldson, intends this summer to cross the Atlantic to Ireland in a large balloon. The machine will weigh about 2,000 lb., will contain 268,000 ft. of gas, with two reservoirs to provide against leakage, and an electrical arrangement for light. The professor calculates to accomplish his trip in from 17 hours to two days and a half, and intends, if the experiment proves successful, to establish a balloon mail and passenger line round the world.

The additions to the Zoological Society's Gardens during the past week included a short-toed eagle (*Circaëtus brachydactylus*), and two Algerian tortoises (*Testudo mauritanica*), from Morocco, presented by Capt. Perry; a white-faced tree-duck, (*Dendrocygna viduata*), and a Capoeira partridge (*Odontophorus dentatus*), from Brazil, and a crocodile from Sumatra, deposited; a Great kangaroo (*Macropus giganteus*), and a vulpine phalanger (*Phalangista vulpina*), born in the gardens; three red-breasted cardinals (*Paroaria culculata*) from South America, and a western ground parrakeet (*Geopsittacus occidentalis*) from South Australia, purchased. Only one specimen of the last mentioned extremely rare bird has been previously alive in the gardens.

SOCIETIES AND ACADEMIES

London

Royal Society, March 20.—"On the Temperature at which *Bacteria*, *Vibriones*, and their Supposed Germs are killed when immersed in Fluids or exposed to Heat in a Moist State." By H. Charlton Bastian, M.D., F.R.S., Professor of Pathological Anatomy in University College, London.

For more reasons than one, we may, perhaps, now look back with advantage upon the friendly controversy carried on rather more than a century ago between the learned and generous Abbé Spallanzani and our no less distinguished countryman Turberville Needham. Writing concerning his own relation to Needham, the Abbé said* :—"I wish to deserve his esteem whilst combating his opinion;" and, in accordance with this sentiment, we find him treating his adversary's views with great respect, and at the same time repudiating much of the empty and idle criticism in which so many of Needham's contemporaries indulged with regard to his work. This criticism Spallanzani says† :—"Without looking into details, contented itself by throwing doubt upon some of the facts, and by explaining after its own fashion others whose possibility it was willing to admit." He moreover warmly reprobated the ignorant and disrespectful statements made by an anonymous writer who had shown himself little worthy of being heard upon the subjects in dispute. Spallanzani on this occasion very wisely said‡ :—"When it is a question concerning observations and experiments, it is necessary to have repeated them with much circumspection before venturing to pronounce that they are doubtful or untrustworthy. He who will allow himself to speak of them with contempt, and who can only attempt to refute them with writings composed by the glimmer derived from a treacherous lamp, will not find himself in a condition to retain the esteem of learned men." The anonymous writer (in his 'Lettres à un Americain"), to whom Spallanzani referred, had gone so far as to doubt the statements of Needham as to the constant appearance of organisms in infusions which had been previously boiled, and also intimated that even if they were to be found, it was only because they had been enabled to resist the destructive influence of the boiling fluid. This latter assertion was emphatically denied by Spallanzani—his denial being based upon a most extensive series of experiments with eggs in great variety and with seeds of all degrees of hardness. These were all found to be killed by a very short contact with boiling water. Spallanzani had thoroughly satisfied himself that even very thick-coated seeds could not resist this destructive agent, whilst he thought that the idea entertained by some, of the eggs of the lowest infusoria being protected from the injurious influence of the boiling water by reason of their extreme minuteness, was a supposition so improbable as scarcely to deserve serious consideration. Such a notion was, he thought, wholly opposed to what was known concerning the transmission of heat. Whilst, therefore, the opinion of those who believe that eggs have the power of resisting the destructive influence of boiling water could be wholly refuted, Spallanzani thought it by no means followed that the infusoria, which always after a very short time appeared in boiling infusions, had arisen independently of the existence of eggs. The infusions being freely exposed to the air, it was very possible that this air had introduced eggs into the fluids, which by their development had given birth to the infusoria.§

After the lapse of a century it has at last been clearly shown that this supposition of aërial contamination advanced by Spallanzani (warrantable and natural as it was at the time) is one which, in the great majority of cases, is devoid of all foundation in fact, so far as concerns the organisms essentially associated with processes of putrefaction, viz., *Bacteria* and *Vibriones*. The means of proving this statement, based upon independent observations made by Prof. Burdon Sanderson and myself, were recently submitted to the consideration of the Royal Society.‖ Before the reading of this communication I was under the im-

* "Nouvelles Recherches sur les Découvertes Microscopiques et la Génération des Corps Organisés, &c. London and Paris, 1769, vol. i. p. 69.

† *Loc. cit.* p. 9.

‡ *Loc. cit.* p. 114.

§ A few pages further on this view is thus shortly expressed. :—"Il est évident que toutes les tentatives faites avec le feu, peuvent bien servir à prouver que les animaux microscopiques ne naissent point des œufs que l'on supposit exister dans les infusions avant qu'on leur fit sentir le feu: mais cela n'empêche, pas qu'ils n'aient pû être formés de ceux qui auront été portes dans les vases après l'ébullition."

‖ See Proceedings of Royal Society, No. 141, 1873, p. 139.

pression that almost every one of those who had taken part in controversies which had been carried on both here and abroad concerning the Origin of Life, were prepared to admit, as Spallanzani had done, that the eggs or germs of such organisms as appear in infusions were unable to survive when the infusions containing them were raised to the temperature at which water boils. This impression was produced in part by the explicit statements on the subject that had been made by very many biologists, and also in part by a comparatively recent and authoritative confirmation which this view as to the destructive effects of boiling infusions upon *Bacteria* had received. Little more than two years ago Prof. Huxley, as president of the British Association for the Advancement of Science, recorded experiments in his Inaugural Address which were obviously based upon this belief as a starting-point. And subsequently, in one of the Sectional Meetings, after referring to some of my experiments, and to the fact that all unmistakeably vital movements ceased after *Bacteria* had been boiled, Prof. Huxley added:*—"I cannot be certain about other persons, but I am of opinion that observers who have supposed they have found *Bacteria* surviving after boiling have made the mistake which I should have done at one time, and, in fact, have confused the Brownian movements with *true living* movements." Prof. Huxley does not now (in reference to the experiments cited in my last communication) suggest that the organisms found in the infusions were dead and had been there before the fluids were boiled: he expresses doubts concerning that which he seems formerly to have regarded as established, and, with much caution, wishes for evidence confirmatory of his own, to show that the germs of *Bacteria* and *Vibriones* are killed in a boiling infusion of hay or turnip, as they have been proved to be in "Pasteur's Solution" and in solutions containing ammonic tartrate and sodic phosphate

With the view of removing this last source of doubt more effectually, and also of refuting the unwarrantable† conclusions of M. Pasteur, to the effect that the germs of *Bacteria* and *Vibriones* are not killed in neutral or slightly alkaline fluids at a temperature of 212° F., I almost immediately after the reading of my last communication commenced a fresh series of experiments.

Nearly two years ago, in my "Modes of Origin of Lowest Organisms," I brought forward evidence to show that *Bacteria*, *Vibriones*, and their supposed germs, are killed at a temperature of 140° F. (60° C.) in neutral or very faintly acid solutions containing ammonic tartrate and sodic phosphate, and also evidence tending to show that these living units were killed in neutral infusions of hay and in acid infusions of turnip at the same temperature.

The crucial evidence adduced concerning the degree of heat destructive to *Bacteria*, *Vibriones*, and their germs, in the saline solution was of this nature. The solution had been shown to be incapable of engendering *Bacteria* and *Vibriones* (under all ordinary conditions) after it had been boiled, although it still continued capable of supporting the life and encouraging the rapid multiplication of any of these organisms which were purposely added to it. Some of this boiled solution, therefore, was introduced into flasks previously washed with boiling water; and when the fluids had sufficiently cooled, that of each flask was inoculated with living *Bacteria* and *Vibriones*—in the proportion of one drop of a fluid quite turbid with these organisms to one fluid ounce of the clear saline solution.‡ These mixtures containing an abundance of living organisms were then heated to various temperatures, ranging from 122° F. (50° C) to 167° F. (75° C.), and it was invariably found that those which had been heated to 122° or 131° became quite turbid in about two days, whilst those which had been raised to 140° F. or upwards as invariably remained clear and unaltered. The turbidity in the first series having been ascertained to be due to the enormous multiplication of *Bacteria* and *Vibriones*, and it being a well-established fact that such organisms when undoubtedly living always rapidly multiply in these fluids, the conclusion seemed almost inevitable that the organisms and their germs must have been killed in the flasks which were briefly subjected to the temperature of 140° F. How else are we to account for the fact that these fluids remained quite unaltered although living organisms were added to them in the same proportion as they had been to those less-heated fluids which had so rapidly become turbid? Even if there does remain the mere possibility that the organisms and their supposed germs had not actually been killed, they were certainly so far damaged as to be unable to manifest any vital characteristics. The heat had, at all events, deprived them of their powers of growth and multiplication, and these gone, so little of what we are accustomed to call "life" could remain, that practically they might well be considered as dead. And, as I shall subsequently show, the production of this potential death by the temperature of 140° F. enables us to draw just the same conclusions from other experiments, as if such a temperature had produced a demonstrably actual death. Seeing also that these saline solutions were inoculated with a fluid in which *Bacteria* and *Vibriones* were multiplying rapidly, we had a right to infer that they were multiplying in their accustomed manner, "as much by the known method of fission, as by any unknown and assumed method of reproduction." So that, as I at the time said,* "These experiments seem to show, therefore, that even if *Bacteria* do multiply by means of invisible gemmules, as well as by the known process of fission, such invisible particles possess no higher power of resisting the destructive influence of heat than the parent *Bacteria* themselves possess."

This is, in fact, by far the most satisfactory kind of evidence that can be produced concerning the powers of resisting heat enjoyed by *Bacteria* and *Vibriones*, because it also meets the hypothesis as to their possible multiplication by invisible gemmules possessed of a greater power of resisting heat, and because no mere inspection by the miscroscope of dead *Bacteria* can entitle us positively to affirm that they are dead, even though all characteristically vital or "true living" movements may be absent.

Facts of a very similar nature were mentioned in the same work, strongly tending to show that *Bacteria* and *Vibriones* are also killed at the same temperature in other fluids, such as infusions of hay or turnip. These facts were referred to in the following statement†:—"Thus, if on the same slip, though under different covering-glasses, specimens of a hay-infusion turbid with *Bacteria* are mounted, (*a*) without being heated, (*b*) after the fluid has been raised to 122° F. for ten minutes, and (*c*) after the fluid has been heated to 140° F. for ten minutes, it will be found that in the course of a few days the *Bacteria* under *a* and *b* have notably increased in quantity, while those under *c* do not become more numerous, however long the slide is kept. Facts of the same kind are observable if a turnip-infusion containing living *Bacteria* is experimented with; and the phenomena are in no way different if a solution of ammonic tartrate and sodic phosphate (containing *Bacteria*) be employed instead of one of these vegetable infusions. The multiplication of the *Bacteria* beneath the covering-glass, when it occurs, is soon rendered obvious even to the naked eye by the increasing cloudiness of the film."

(*To be continued.*)

Geological Society, March 12.—Joseph Prestwich, F.R.S., vice-president, in the chair. The following communications were read:—1. Note on some Brachiopoda collected by Mr. Judd from the Jurassic deposits of the East Coast of Scotland, by Thomas Davidson, F.R.S. In this note the author stated that four species of Brachiopoda collected by Mr. Judd were especially worthy of notice, two of them being quite new, and two new to Britain. Three of them were obtained from the equivalent of the Kimmeridge clay, which was the more remarkable as the Brachiopoda of that formation are comparatively few. The new species described were *Rhynchonelle Sutherlandi* and *Terebratula Joassi*, derived, with *Terebratula humeralis* Röm., from the Upper Oolite of Garty in Sutherland; the fourth species is *Terebratula bisuffarcinata* Schlot., from the Lower Calcareous Grit of Bramberry Hill. 2. On Solfataras and deposits of Sulphur at Kalamaki, near the Isthmus of Corinth, by Prof. D. T. Ansted, F.R.S. After noticing the traces of volcanic action east of the Pindus chain, the author described the Solfataras and sulphur-deposits of the neighbourhood of Kalamaki as furnishing indications that there is even now a real though subdued volcanic energy in this part of Europe. 3. On the origin of clay-ironstone, by Mr. J. Lucas, F.G.S. The author commenced by giving a general view of the varieties, chemical composition, and mode of occurrence of clay-ironstone, and suggested that the formation of all the bedded varieties may be explained by the

* See Report in Quart. Journ. of Microscop. Science, Oct. 1870.

† Reasons for this opinion have been fully set forth in "The Beginnings of Life," p vol. i. 374 *et seq.*; or the discriminating reader may at once find my justification for this expression by reading pp. 58-66 of M. Pasteur's memoir in Ann de Chim. et de Physique. 1862.

‡ Fuller details concerning these experiments may be found in the little work already mentioned at pp. 51-56, and also in "The Beginnings of Life," vol. i., pp 325-332.

* "Modes of Origin of Lowest Organisms," 1871, p. 60.

† *Loc. cit.*, p. 60.

supposition that they originated in peaty or non-peaty lagoons on the alluvial flats of the deltas of the Carboniferous formations, which would present semi-terrestrial conditions, that is to say, a surface exposed to the air but subject to be covered by floods. 4. Note in vindication of *Leptophlœum rhombicum* and *Lepidodendron gaspianum*, by Principal Dawson, LL.D., F.R.S. This note accompanied some photographs of the remains of plants referred to, and was in opposition to the identification of these remains with the *Lepidodendron nothum* Unger, as proposed by Mr. Carruthers in his Appendix to Mr. Daintree's paper on the Geology of Queensland.

Zoological Society, March 18, 1873.—The Viscount Walden, F.R.S., president, in the chair.—A communication was read from Mr. R. B. Watson on some marine mollusca from Madeira, including a new genus of the *Muricidæ*, proposed to be called *Chascax* and a new *Rissoina*, and embracing descriptions of the whole of the *Rissoæ* of the group of islands.—A communication was read from Dr. J. D. Macdonald, F.R.S., on a specimen of *Acanthias vulgaris* and a species of *Galeus*, probably new to science, taken off Flinder's Island, Bass' Straits.—Mr. W. T. Blandford read a paper on the Gazelles of India and Persia. This contained the description of a new species, *Gazella fuscifrons*, founded on a single specimen obtained by the author in 1872, near the edge of the desert of Seistan.—A communication was read from Dr. J. S. Bowerbank, F.R.S., containing the fifth part of a series of memoirs entitled Contributions to a General History of the Spongiadæ.—A communication was read from Mr. Gerard Krefft, C.M.Z.S., containing the description of a new species of crocodile from Queensland, proposed to be called *Crocodilus johnsoni*.—Mr. Edward Bartlett exhibited and gave the description of a new moth belonging to the family Saturniidæ, which had been obtained in the interior of Madagascar by Mr. T. Waters, and which was proposed to be called *Tropæa madagascariensis*.

Mathematical Society, March 13.—Dr. Hirst, F.R.S., president, in the chair.—Prof. Greenhill, of Cooper's Hill College, was elected a member.—Mr. R. B. Hayward read a paper on an extension of the term *area* to any closed circuit in space. In the sense in which the writer employed the term, area is no longer a mere magnitude or a magnitude affected only with the positive or negative sign, but a magnitude affected with direction; in other words it is a *vector*, not simply a *scalar*. The paper concluded with a few illustrations of the use of this extension of the term area.—Other communications were, on the evaluation of a class of definite integrals involving circular functions in the numerator and powers of the variable only in the denominator, by Mr. J. W. L. Glaisher; note on normals and the surface of centres of an algebraical surface, by Mr. S. Roberts, V.P.; and a proof of the proposition that a number which divides the product of two numbers and is prime to one of them will divide the other, by Mr. M. Jenkins (Hon. Sec.).—Notice was taken in NATURE (August 1, 1872) of the formation of a mathematical society in Paris on the plan of the similar societies of London, Moscow and Berlin. This society having forwarded the first number of its "Bulletin," it was agreed to exchange publications.

Chemical Society, March 20, Dr. Frankland, F.R.S., president, in the chair.—Mr. C. W. Siemens, F.R.S., delivered a lecture "On Iron and Steel." The lecturer, after adverting to his former discourse delivered before the Society in 1868, and describing the various experiments he had made to obtain malleable iron direct from the ore, gave an account of the process by which he had succeeded in completely attaining that object. It consists essentially in fusing the ore by means of the most intense heat in a revolving furnace, and then adding the requisite amount of carbonaceous matter to reduce the iron to the metallic state. The malleable iron thus precipitated in the molten mass becomes aggregated into balls by the revolution of the furnace, and can then be easily removed. It is free from sulphur, phosphorus, and other impurities, and dissolves readily in a bath of molten cast iron, producing steel of a quality equal to that made from the best Swedish bar iron.

Anthropological Institute, March 18.—Prof. Busk, F.R.S., president, in the chair. A paper was read by Mr. George Harris, F.S.A., on theories regarding intellect and instinct, with an attempt to deduce a satisfactory conclusion therefrom. The author, after taking a general survey of the opinions on this subject, citing those of Aristotle, Plato, Descartes, Hobbes, Locke, and several other writers, including some modern authorities, proceeded to compare them one with another, and to consider how far certain apparently irreconcileable differences might be considered compatible. The great perfection of the sensitive system in animals he considered to be the main cause of the unerring dexterity with which they engage in various operations connected with their career. And although they differ essentially from man as regards his capacity for abstract studies, it appears difficult to deny to them the possession of an immaterial being of some kind. High authorities, both among philosophers and divines, have attributed to them a future state of existence. Mr. Harris also read a paper on the concurrent contemporaneous progress of renovation and waste in animated frames, and the extent to which such operations are controllable by artificial means. The writer took a general view of the opinions of those who have treated on this subject, more especially the older authorities, citing Galen, Willis, Buffon, Hunter, and Smellie, and referring also to recent articles on the subject in *Fraser's Magazine* and the *Edinburgh Review*. He adverted to the ascertained fact of the progress of renovation and waste in all animated frames, as also to the circumstance that certain of these operations were known to be controllable. He analysed the principle of waste and decay in different bodies, and referred to ossification of the bones and deterioration of the blood as contributing to those conditions. As medical science advances these matters might be more perfectly understood. He recommended experiments of various kinds as to the nature of substances, and their effect on bodies animate as well as inanimate, and with regard to animals and plants as well as man.

Royal Horticultural Society, March 19.—Scientific Committee, Dr. J. D. Hooker, C.B., in the chair. Prof. Thiselton Dyer called attention to the discovery by Fankhauser of the prothallial stage of *Lycopodium*. It appears to be almost identical with that of the *Ophioglosseæ*, and consequently a totally different from that of *Selaginella*. It was remarkable that the carboniferous *Lepidostrobus* and *Triplosporites* differed as regards their spores in precisely the same way as *Lycopodium* and *Selaginella*. If the nature of the germination in the two latter must be held to imply systematic diversity, analogy would equally imply it in the case of the two former. But the parallelism would, under these circumstances, be extremely difficult to understand.—General Meeting, W. Wilson Saunders, F.R.S., in the chair. The Rev. M. J. Berkeley commented on the fine collection of *Cycadeaceæ* exhibited by Mr. Bull, a well-fruited pot-plant of the Loquat (*Eriobotrya japonica*), and *Epidendrum erubescens*—a Guatemalan orchid rarely seen in flower, which was exhibited by Mr. C. Leach.

Entomological Society, March 17.—Prof. Westwood, president, in the chair.—M. Ernest Olivier was balloted for and elected a foreign member.—The president exhibited a very rare species of *Paussus* from Abyssinia.—Mr. Smith exhibited a box of ants sent from Calcutta by Mr. G. A. J. Rothney, collected principally in the Botanic Gardens. There were many new species amongst them, a complete series of which was to be reserved for the national collection.—Mr. Cole exhibited two boxes of *Bombycidæ* from Natal.—Mr. Bates read a paper on some species of geodephagous coleoptera from China.—Mr. Müller made some remarks on a beetle (*Arrocerus coffeæ*) which had been imported into Basle with some coffee from Java, and that the insect had since become naturalised and might be found in any quantity there. Mr. Müller also remarked on a cargo of ground nuts which arrived in London direct from Sierra Leone, the kernels of which were destroyed by myriads of the larvæ and perfect insects of the *Tribolium ferrugineum*, accompanied by the larvæ and perfect insects of a species of *Rhizophagus* preying on the former.—Mr. Dunning read some further notes on *Atropos pulsatoria*, with reference to Dr. Hagen and Mr. W. A. Lewis. Mr. Bates put some questions to the meeting, suggested to him by Mr. Darwin, with a view to eliciting information as to sexual differences in certain insects, viz., whether any cases had been noticed of sexual differences in the ocellated spots with which certain insects, as the *Bombycidæ*, were furnished, and also as to sexual differences amongst the *Buprestidæ*. A conversation ensued during which Mr. Jenner Weir stated that *Satyrus hyperanthus* had more ocellated spots in the female than in the male; and Mr. Butler mentioned that *Drusillus* had double ocelli in one sex. It was also stated that Mr. Saunders had detected sexual differences among the *Buprestidæ*.

MANCHESTER

Literary and Philosophical Society, March 4.—Dr. J. P. Joule in the chair.—Mr. Baxendell read the following communication

from Mr. S. Broughton:—It appears there is some doubt as to the existence of ball discharge in thunderstorms. At the request of Mr. Baxendell I communicate an observation of such, seen during the approach of a storm, in 1854 or 1855, when walking from Altrincham to Timperley. Over the edge of a cloud near the east horizon a flash of lightning was seen, and a ball *apparently* the size of one from a Roman candle shot upwards through an arc of 20° or 30°. I cannot say that it went to another cloud, but that would most likely be so, as my attention was taken up watching the progress of the electric ball.—E. W. Binney, V.P., F.R.S., said that shortly after the meeting of the Society on January 21, when he exhibited the singular fossil plants, which were quite new to him at the time, which he thought would have to be placed in a new genus, he had received excellent transverse and longitudinal sections of similar specimens from Professor Renault of Cluny, which were if possible in a more beautiful state of preservation than those found in the carboniferous strata of Lancashire. On February 4, Prof. W. C. Williamson, F.R.S., stated that these specimens were the branches or stems of the well-known genus *Asterophyllites*. Now the French professor states that he had described this fossil plant in a memoir read before the Academy in 1870, and that in his opinion it belonged to *Sphenophyllum*. I am not in possession of the facts from which the two learned professors came to such different conclusions, but I am inclined to consider the singular little stem as belonging to a new genus until the leaves of *Sphenophyllum* or *Asterophyllites* are found attached to it. When this comes to pass of course there can be no doubt on the matter.—The President said that he had made another observation of the position of the freezing poin in the thermometer used in making the observations recorded in the Proceedings for April 16, 1867, and February 22, 1870. The gradual rise of the zero during twenty-nine years was shown by a diagram, the ordinates representing divisions etched on the glass stem, each corresponding to $\frac{1}{1\cdot5}$ of a degree Fahrenheit.—Mr. William H. Johnson, B.Sc., read a paper "On the Influence of Acids on Iron and Steel," in which he showed the general effects of acid; its effects on the weight; on the breaking strain and elongation; effect of pyroligneous acid; effects of acids on copper and brass; and of zinc on iron.

PARIS

Academy of Sciences, March 17, M. de Quatrefages, president, in the chair.—The following papers were read: On the theory of the movement of Jupiter, by M. Le Verrier.—The transit of Venus—method for obtaining the moment of contact by photography, by M. Janssen. The author suggests the use of a photographic plate cut in the form of a disc, and made to revolve. By this means a number of photographs can be obtained with very minute intervals of time between each exposure.—On the heat produced by the mixture of the hydracids with water and on the molecular volumes of their solutions, by M. Berthelot. The acids experimented on were the hydrochloric, hydrobromic, and hydriodic. The author decides that these acids and their compounds give rise to similar amounts of molecular work.—On new applications of the principles of the navigation sluice to oscillating columns of liquid, by M. A. de Caligny.—On a shock of earthquake observed at Florence on March 12, 1873, by M. de Tchihatchef. The shock was observed at 9h. 5m. p.m., it did not last more than half a second, and its direction was S.E. to N.W., bar. 725mm.—M. Secchi presented his memoir "On the Distribution of the Prominences on the Solar Disc, and on the study of the Spots."—On barometic changes and their connection with magnetic variations, by M. J. A. Broun.—New experiments on singing flames, by M. F. Kastner.—Observations on the theory of solar cyclones, by M. E. Vicaire. The author raised several objections to M. Faye's theory of the sun, and promised to explain his own hypothesis shortly; this, he said, was simply that of Wilson.—On "Spectrometry;" Spectronatrometry, by MM. P. Champion, H. Pellet, and M. Grenier. The authors described an instrument for the spectroscopic estimation of minute quantities of sodium. The principle depended on the comparison of a sodium flame in which a known quantity of sodium was being heated with the flame coloured by the substance the sodium in which it was required to know. The apparatus described was somewhat complicated, but the principle upon which it worked was the use of a graduated compensating wedge of coloured glass. M. Janssen made some observations on the process.—Observations on M. Gernez's recent note on the crystallisation of supersaturated solutions, by M. Ch. Violette.—On the methods of increasing the length of bones and stopping their growth, by M. Ollier. On the anatomy of *Comatula rosacea*, by M. Edm. Perrier. On a deposit of fossil mammiferæ near Lapsista, Macedon, by M. Gorceix. "On polyhedric concamerations," by M. G. Perry.

DIARY

THURSDAY, MARCH 27.

ROYAL SOCIETY, at 8.30.—The Radiation of Heat from the Moon, the Law of its Absorption by our Atmosphere, and of its Variation in Amount with her Phases (Backerian Lecture): Earl of Rosse.
SOCIETY OF ANTIQUARIES, at 8.30.—Election of Fellows.
ROYAL INSTITUTION, at 3.—Coal and its Products: A. V. Harcourt.

FRIDAY, MARCH 28.

ROYAL INSTITUTION, at 9.—Force and Energy: Prof. Clifford.
QUEKETT CLUB, at 8.
ROYAL COLLEGE OF SURGEONS, at 4.—Extinct Mammals: Prof. Flower.

SATURDAY, MARCH 29.

ROYAL INSTITUTION, at 3.—Darwin's Philosophy of Language: Prof. Max Müller.

MONDAY, MARCH 31.

LONDON INSTITUTION, at 4.—Fungoid Organisms: Prof. Thiselton Dyer.

TUESDAY, APRIL 1.

ROYAL INSTITUTION, at 3.—Forces and Motions of the Body: Prof. Rutherford.
ANTHROPOLOGICAL SOCIETY, at 8.—Notes on the Collection of Peruvian Skulls and Pottery lately received from Consul Hutchinson: Prof. Busk and Dr. Barnard Davis.—On the Natives of Vancouver's Island: Richard King.—On a Human Skull from Birkdale, Southport: T. M. Reade.
SOCIETY OF BIBLICAL ARCHÆOLOGY, at 8.30.
ZOOLOGICAL SOCIETY, at 8.30.—On the Brain and a portion of the nervous system of *Pediculus capitis*: Dr. J. S. Bowerbank.—Notes on the genera of Turtles (*Oiacopodes*) and especially on their skeleton and skulls: Dr. J. E. Gray.—Descriptions of three new species of Flying Squirrels: Dr. A. Günther.
ASIATIC SOCIETY, at 3.

WEDNESDAY, APRIL 2.

SOCIETY OF ARTS, at 8.—On Economy of Fuel for domestic purposes: Capt. Douglas Galton, C.B.
LONDON INSTITUTION, at 7.—Courts of Special Commercial Jurisdiction: N. H. Paterson.
ROYAL MICROSCOPICAL SOCIETY, at 8.—On a new *Callidina* with the result of experiments on the desiccation of Rotifers: H. Davis.—On the Development of the Sturgeon's facial arches: W. K. Parker.

THURSDAY, APRIL 3.

CHEMICAL SOCIETY, at 8.—A way of exactly determining the specific gravity of Liquids; Dr. H. Sprengel.—On Cymene from various sources: Dr. C. R. A. Wright.—Researches on the action of the Copper-zinc couple on organic bodies, II.—On the iodides of Amyl and Methyl: J. H. Gladstone and A. Tribe.—Contributions from the Laboratory of the London Institution, No. XI.—Action of the acid chlorides on Nitrates and Nitrites: Dr. H. G. Armstrong.
LINNEAN SOCIETY, at 8.—On new Indian Fishes: Surgeon-Major F. Day.—On the Fungi of Ceylon: Rev. M. J. Berkeley and C. E. Broome.
ROYAL INSTITUTION, at 3.—Coal and its Products: A. V. Harcourt.

BOOKS RECEIVED

ENGLISH.—Celestial Objects for Common Telescope. 3rd edit.: Rev. T. W. Webb (Longmans).—Elementary Treatise on Wave Theory of Light. 3rd edit.: H. Lloyd (Longmans).—The Childhood of the World: E. Clodd (Macmillan).

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